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THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

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IN

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Della Clorosi commentario di Carlo Speranza, Professore de Terapia speciale, e di Clinica interna nella Ducale Università di Parma. Milano, 1828. Pp. 95.

Caso singolare di un Cadavere sudante, e riflessioni sul Medesimo, letto alla Società Medico-Fisico Fiorentina da Carlo Speranza. Bologna, 1828. Pp. 20.

Guarigione di varie Febbri Intermittenti col Solfato de Chinina applicato col Metodo Endermico di Carlo Speranza. Bologna, Pp. 16.

A Practical Treatise of the Diseases of the Eye. By William M'Kenzie, Lecturer on the Eye in the University of Glasgow, &c. &c. London, 1830. 8vo.

A Supplement to the Pharmacopœia, and Treatise on Pharmacology in general: being a complete Dispensatory and Manual for Medical Practitioners and Retail Druggists. By Samuel F. Grey, Lecturer on the Materia Medica, Pharmaceutical Chemistry and Botany. London, 1831. 8vo.

A Treatise on Auscultation, illustrated by Cases and Dissections. By R. Spittal, Physician's Assistant to the Royal Infirmary of Edinburgh, &c. &c. Edinburgh, 1830. 8vo. Pp. 280.

Observations on Mental Derangement. By Andrew Combe, M. D. Edinburgh, 1831. 8vo.

The Physiology of the Fœtus, Liver and Spleen. By George Calvert Holland, M. D., Lecturer on Physiology, and on the Practice of Physic in the Sheffield Medical Institution. London. 4831. 8vo. Pp. 229.

Manual of Operative Surgery, translated from the Third Edition of the French of J. Coster, M. D. P. By George Fife, M. D., Surgeon to the Northern Public Dispensatory. Edinburgh, 1831. 8vo. Pp. 408.

The Medical Annual for 1831. By R. Reece, M. D., F. R. S. L. London, 1831. 8vo. Pp. 124.

The Medico-Chirurgical Review, and Journal of Practical Medicine. Edited by James Johnson, M. D., &c. No. 27, January 1831.

The London Medical and Surgical Journal: including the London Medical Repository, exhibiting a View of the Improvements in the various branches of Medical Science. Edited by Michael Ryan, M. D., &c. Nos. 30-33. December, January, February, and March. London, 1831.

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The London Medical Gazette from December 11th to March 12th. From No. 158 to No. 171.

The American Journal of the Medical Sciences. No. 13. November.

Engravings of the Arteries of the Human Body, from Tiedemann. By E. Mitchell. No. 27 to No. 32.

Illustrations of Surgical Anatomy. By John G. Burt, Surgeon to the City Dispensary, &c. &c. Nos. 1 and 2. Edinburgh, 1831.

The North American Medical and Surgical Journal. Nos. 19 and 20. Philadelphia. 1830.

Monthly Gazette of Practical Medicine. By R. Reece, M. D. F. R. S. L. Nos. 1 and 2. 1831.

Medical Zoology and Mineralogy. By John Stephenson, M. D. F. L S. No. 1. London, 1831.

Illustrations of Mr S. Cooper's Surgical Dictionary. Published monthly. London, 1830. Parts 1, 2, 3, 4, 5, and 6.

The Anatomical Atlas of Dr M. J. Weber, Professor at the Royal Prussian University of Frederick William, at Boon. Nos. 1 and 2: containing 16 Plates, with explanation. London, 1831. Published by A. Schloss.

Anatomical Demonstrations; or Colossal Illustrations of Human Anatomy. By Professor Seerig. Translated from the German. Part 1, with Plates. London, 1831. Published by A. Schloss.

Report of the Royal Dispensary for the Diseases of the Ear, Dean Street, Soho Square, from 1817 to 1830. By John Harrison Curtis, Esq. Surgeon Aurist to his Majesty, &c. &c. London, 1830.

Lecture introductory to a Course of Clinical Surgery delivered to the Students of the Glasgow Royal Infirmary. By M. S. Buchanan, M. D. Surgeon to the Royal Glasgow Infirmary, &c.

An Introduction to the Study of Human Anatomy. By James Paxton, Member of the Royal College of Surgeons. With Illustrations. London, 1831.

The effects of the Principal Arts, Trades and Professions, and of Civic States and Habits of Living on Health and Longevity: with a particular reference to the trades and manufactures of Leeds, &c. By C. Turner Thackrah. London, 1831.

ERRATUM.

For " incorrect" *read* " concurrent," in page 276, line 26.

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A Treatise on the Nature and Cure of those Diseases, either acute or chronic, which precede Change of Structure, with a view to the Preservation of Health in Organic Diseases. By A. P. W. Philip, M. D., F. R. S. L. and E., &c. London, 1830. 8vo.

Lectures on Anatomy; interspersed with practical Remarks. Vol. II. By Bransby B. Cooper, F. R. S. Surgeon of Guy's Hospital, Lecturer on Anatomy, &c. &c. &c. London, 1830. 8vo.

Inquiries concerning the Intellectual Powers and the Investigation of Truth. By John Abercrombie, M. D., &c. &c. &c. Edinburgh, 1830. 8vo.

Appendix of the Second Edition of a series of Observations on Strictures, &c. &c. By R. A. Stafford. London, 1830. Pp. 52.

An Address introductory to a Course of Lectures on the Principles and Practice of Physic. By James Baker, Surgeon. London, 1830. Pp. 22.

An Inquiry respecting the period of Puberty in Women. By John Robertson, Esq. Surgeon to the Lying-in Hospital, Manchester. Pp. 30.

Practical Remarks on the Nature and Effects of the Expressed Oil of the Croton Tiglium; with Cases illustrative of its Efficacy in the Cure of various Diseases. By Michael John Short, M. D. London, 1830. Pp. 63.

Modern Medicine influenced by Morbid Anatomy. An Oration delivered at the 57th Anniversary of the Medical Society of London, also, An Apology for Medical Nomenclature. By Leonard Stewart, M. D., &c. &c. &c. London, 1830. Pp. 50.

Views of the Pelvis, showing the natural size, Form, and Relations of the Bladder, Urethra, Rectum, Uterus, &c. &c. in the Infant and in the Adult. By John Houston, Demonstrator of Anatomy in the Royal College of Surgeons in Ireland, &c. Dublin, 1829.

Cases illustrative of the Efficacy of Various Medicines administered by Inhalation, in Pulmonary Consumption, &c. By Sir Charles Scudamore, M. D., F. R. S. &c. &c. &c. London, 1830. 8vo.

Remarks on a New and Important Remedy in Consumptive Diseases, with a description of an Apparatus for the Easy and Domestic employment of a Medicated Atmosphere in cases of Impeded Respiration. By John Doddridge Humphreys. London, 1831. 12mo. Pp. 50.

The Medico-Chirurgical Review, and Journal of Practical Medicine. Edited by James Johnson, M. D., &c. No. 26, October 1830.

The London Medical and Surgical Journal; including the London Medical Repository, exhibiting a view of the Improvements in the various branches of Medical Science. Nos. 28 and 29, October and November. Edited by Michael Ryan, M. D. &c. London, 1830.

The London Medical Gazette, from September 11th to December 4th. From No. 145 to No. 157.

The Maryland Medical Recorder, devoted to Medical Science in General. Conducted by Horatio G. Jameson, M. D. Professor of Surgery in Washington Medical College, Baltimore. Vol. i. No. 1, September, 1829. Baltimore.

The American Journal of the Medical Sciences, No. 11, May 1830. No. 12, August 1830.

Engravings of the Arteries of the Human Body from Tiedemann. By E. Mitchell. No. 20, 21, 22, 23, 24, 25, 26.

The North of England Medical and Surgical Journal, No. 1. August 1830.

Gazette Medicale de Paris, Journal de Médecine et des Sciences Accessoires, Nos. 8—30.

In consequence of many of our readers complaining of the minuteness of the character of the Medical Intelligence, and thereby rendering perusal difficult, we have been induced to alter it to one larger. No change, however, will be made in any other respect, unless, perhaps, to render the Intelligence still more extensive, as we shall then feel less compunction for the eyes of our readers, when not compelled to gaze at closely printed lines of small type. In other respects, the full proportion of this department will be given, corresponding to that of the small type.

M^r. WATSONS ESSAY.

Edin^r. Med. Journ. N^o 106 p.

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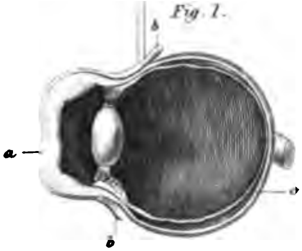

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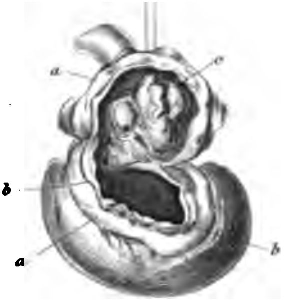

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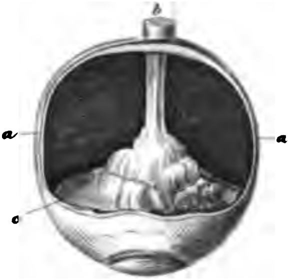

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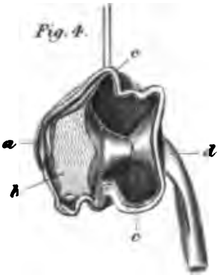

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1. JANUARY 1831.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Sketch of the State and Practice of Medicine at Constantinople.* By C. BRYCE, M. D.

THE influence which Islamism exercises over the minds of its proselytes, explains the reason why medicine should have retrograded, or at least had its progress arrested, in Turkey, and why its native medical science is only the remains of former times, disfigured and disguised under a thousand forms. And though the murmurs of affliction, the solicitation and avidity for advice and remedies, with which an European is assailed on his first professional intercourse with Turks, may seem incongruous with reliance on the dogmas of fatalism, yet they cannot be regarded as contradicting this opinion. Further observation and inquiry discover, that this pervading spirit proceeds rather from indifference to their creed, and an extravagant and false impression of the virtues of drugs, than from a just sense of the attributes of the physician; and therefore it is, that, on the one extreme, the Egyptian, Asiatic, and lower classes of Turks of the capital, resign with ignorant confidence their calamities to the dispensations of Providence, or only resort to curative means when their mental energies are subdued; and that, on the other, the majority, especially of the higher classes, who commonly disguise any scruple of con-science under the assumption that their ailments are merited chastisements for indulgence in licentiousness, revere medicine, if correctly applied, as an infallible means of restoration. This explains the inconsistence in the Osmanli character and principle; and we can distinguish betwixt the Mussulman in his state of native prejudice, and the Mussulman whose ideas and feelings have been controlled by the revolution of opinion, manifestly expressed of late years in the capitals of Turkey and Egypt.

The chief source of medical practice amongst Turkish practitioners springs from that spirit of charity which so largely pervades Islamism, and of which the foundation of hospitals and the establishment of schools forms a prominent feature. Of the former there are several at Constantinople either endowed by revenues from Mosques, or directly supported by government, of which the medical attendants are Turks, under the appointment and control of the Hakim Bashi, or first physician to the Sultan. Their internal economy and medical management are, it is true, alike defective, if judged by the double object we propose in such establishments, a charitable institution and a school for instruction; but their immediate general usefulness is not less conspicuous, and their existence affords the ready means at any future period of extending their advantages. The madhouses, although disgustingly kept, and offering the most deplorable objects to the curiosity of the stranger, show how wisely the prophet made his scheme of legislative religion subservient to the calls of humanity. Of the schools at Constantinople one is particularly dedicated to medical science, or, more correctly, was,—for, if actually not altogether abandoned, its object is entirely superseded by the new school lately opened, of which I shall afterwards speak. The only previous preparations for this study required of the students, who on an average amounted to thirty, was a competent knowledge of the Arabic, in which language, under the direction of Turkish teachers, they read, and learnt to explain the ancient Egyptian and a few European authors. In this course of instruction, which occupied two hours daily, little if any reference was made to anatomy or chemistry; and even attendance at hospitals was not comprised. Six or seven years were thus employed, when, without any form of examination, and by a simple licence or permission from the Hakim Bashi, the doctoral bonnet might be assumed, and the person was held qualified for the first professional employments of the state. The present Hakim Bashi and his brother, physician in chief of the army, are from this school.

From my observations and inquiries amongst this class of practitioners, the study of medicine is reduced to the knowledge of a few doctrines; and the practice thence promulgated accords with this simplicity. The leading principles are to recognize only three classes of diseases; depraved humours, sudden cold, and great heat; and accordingly, to admit three forms of cure; purgatives, heating medicines, and refrigerants. Other Turkish nosologists adopt a different system, in which all diseases are distinguished into three orders,—namely, *Nevasil,*—*Mayaxil,*—and *Yel.* To the first, which answers to the genus *Caturrhus* of the ancients, belongs almost every disease which has its seat in the head, throat, and thorax. Diseases of the abdomen and affections of the skin are ranked in the second order, which may be translated *hemorrhoidal.* The *Neuroses*, whether or not accompanied by *pyrexia*, affections of tendinous parts, &c. are placed in the third class. *Yel* signifies wind; and to its presence and inordinate localities are ascribed the most severe and the most opposite complaints. Other incongruous ailments, not assorting easily with these genera, are implicitly believed to be the effect of Satanic influence; and the treatment of these is, as it should be, entrusted to churchmen, who make, as in other more enlightened countries, a lucrative business, by offering prayers, and employing exorcism for the deliverance of those affected.

In prescribing, vegetable preparations are usually preferred, which, as indeed every quality of drug, is administered in the simplest form. It must, however, be said that if their drugs are subjected to few alterations in the laboratory, yet in the physician's hands they are compounded in the most ignorant manner, medicines of very opposite virtues being combined in a prescription of fifteen or twenty different drugs; and the more lengthy the writing, the more complex the remedies, the more highly are the skill and learning of the prescriber estimated. Solid purgatives of the strongest kind are freely administered, the more so that Moslemin have a great repugnance to enemata. Syphilis has been long successfully treated by purgatives and sudorifics, joined to the use of the vapour-bath, continued during twenty or thirty days, or until the patient be much exhausted; and this condition is considered as indicating a speedy cure. Emetics are little used, from a great aversion to vomiting. An opinion of a vitiated state, or false course of the blood, enters largely into their explanation of the cause of diseases; and bleeding, either general or local, fortunately is abundantly pursued; though much of the efficacy of the former is deemed to depend on the nicety with which the physician distinguishes what vein should be opened.

But it is not to the application of rules, or the exhibition of drugs alone, that the Ottomans confide in their treatment of maladies. Ignorance makes them reverence a variety of superstitious remedies; and Turks holding the highest posts of learning and rank are not ashamed to employ openly these productions of fanaticism and charlatanism. Of this credulity religion is the common basis; and it is matter of daily occurrence to find the Iman supplant the physician, or at least his mediation sought to aid the virtue of the prescription. The practice is varied according to the caste of the Dervisch, and qualified with pomp and ceremony corresponding to the quality of the invalid or price paid. To some the practitioner prescribes a course of several days reading from the Koran, with certain forms of breathing on the patient by one or more of the initiated; while others are ordered to combine the swallowing of bits of almonds, or of pork, eating off particular plates, having words written on them, and in fevers to tie knotted threads on the wrists and ancles.

These simple plans of empiricism, or of gross superstition, are frequently abandoned by the people, especially in lower complaints, for a treatment that so closely resembles magnetism, that, were it not for its more distant origin, it might be pronounced a variety of that process. The operator consulted listens to the case with the utmost seriousness, refers to his Koran, and having chosen and written an appropriate passage, folds it in a mysterious manner, and prescribes this to be worn in contact with the part affected; after which, the fingers smeared in saliva are stroked over the supposed seat of disorder, and prayers are recited at three intervals. As Dervisches and Imans are the privileged in this function, it is not to be wondered at if it exercise some influence over Osmanlies; but Christians and Jews likewise have recourse to these magnetizers, who change nothing of their spiritual remedies in regard to Greek, Armenian, or Jew. It is singular enough that the saliva and breathing are used in imitation of Christ resuscitating the dead child, and restoring hearing to the deaf man by anointing with spittle. Patients are often directed to swallow several times a-day, slips of paper on which the name Allah or Mahomet has been written by some venerated priest or hadgi. Much faith is also placed by Mussulmans in the efficacy of water sanctified by holding in solution passages of the Koran, which an Iman has traced with ink on the inside of the cup; and, incredible and absurd as it may seem, it has happened to several European practitioners in Turkey to find on their second visit that the true believer has deemed swallowing the prescription left him more advisable than sending it to the apothecary. In further illustration of this spirit of fanaticism, I may add that I saw in Egypt, and the caste may be traced throughout Turkey, Arabian conjurors, whose name corresponds to Messiah, from whom they pretend to derive their science as chief of the sect. Their peculiar province is to neutralise by charms the venom of serpents and scorpions, and render innoxious the bite even of the Abushabat.

Besides the drugs prescribed to alleviate sickness, many more are used in health, from the influence of climate or education, rendering people unsatiable of voluptuousness. Some are used to acquire plumpness and beauty of the skin, as mucilages, feculent fruits, and seeds; others, as cosmetics, composed of fine oils, the fat of animals, odorous resins, alkaline and metallic soaps, prepared with great care and by difficult processes. But the virtue of these remedies, and the chief cause of their being sought after with such avidity, proceeds from their being always mixed with various excitants and stimulants. The most valued variety, however, of this class of drugs and compositions is that used with the intention of affording enjoyments, whether real or ideal. The common sort is that formed from opium, hellebore, and strong aromatics. The more rare and expensive is the *philonium*, consisting of fine opium with ambergris, musk, essential oils, and saffron, and the theriac.

The Egyptians boast that they are the only possessors of the latter, and accordingly prepare it publicly and with great solemnity at Cairo, where it forms a considerable article of commerce, being thence carried to all parts of Asia, and where the profits of its sale are devoted to charitable institutions. The immediate effects of all these mixtures are to stimulate the nervous system, to dissipate melancholy, and give courage and confidence, and during artificial sleep to procure that state of joyous dreamy delirium in which the imagination dwells on pleasant images only. But these transient moments of ecstacy are followed by hours of distress revolting to humanity. The dismal scenes I viewed from the abuse of these agents in Egypt, however vividly impressed on my mind, seemed far below the depravity to which it has reduced the theriacs of Constantinople. Their physical powers are only the shadow of being, and their mental, when roused by the largest doses, scarcely evince the faculty of thought or feeling.

In this sketch of the state and practice of physic peculiar to the Turks, and restricted to a small proportion of the profession at Constantinople, the larger towns of the empire, or even in the interior of the country, we lament the unimproved condition of the simplest, and the prevalence of superstition in the more complex, forms of its exhibition. But a different feeling must move us in our exposition of the state of medical practice in the hands of

Greeks and Jews. In the former, the errors of ignorance prevailed, and were more prejudicial to science than to humanity; among the latter, the cunning of charlatanism predominates, mocking the character of physician under the most interested guises, and degrading the profession to a level with the meanest handicraft. To illustrate this seemingly harsh sentiment, it is necessary to premise that the Turkish administration, by a singular anomaly, gives a free license to all foreigners to proclaim themselves doctors without being subjected to any examination, or required to exhibit testimonials of their studies or qualifications; and hence, although the proportion of practitioners, of one description or other, at Constantinople, exceeds that of any European capital, yet not twenty of this number could be selected who have received a regular medical qualification. This extraordinary absence of restrictive laws probably arose from the scarcity, if not absence, of native professional talents, and the insufficiency of the means of obtaining medical knowledge; and it is continued by the partiality shown to Europeans; add to which, that the title of physician is held in the highest esteem by Turks, even to the rendering the name of Giaour acceptable if so qualified, and that the moral disposition and uniform ignorance of Turks, Armenians, and Greeks, disposes them to yield, without inquiry, a ready confidence to any one who enjoys reputation false or just, or who, by a certain mannerism adapted to their capacities, knows how to render himself agreeable to their peculiarities. The consequence of these inducements and privileges is, that general practice, and in some instances the most lucrative and respectable, is engrossed by people whose pretensions to medical knowledge are alone authorized by their self-adoption of the doctoral bonnet.

These medical characters may be considered under two classes, the one embracing those individuals who have never left Constantinople, the other those who have spent some time at a foreign university. The education of a portion of the former consists in attending the visits of a physician as Dragoman or interpreter for some years, and thereby acquiring the knowledge of that style of explaining diseases, and reconciling remedies adapted to Turkish pathology; and subsequently, in applying to an apothecary for the phraseology of receipt-making. For this instruction Turkish practice affords particular facility; for whilst the physician has his pipe and coffee, the patient discourses on his ailment and treatment with an inquisitiveness which it requires great ingenuity to satisfy. Others act as druggists for a period, and, when sufficiently known in that capacity, find the transit easy into the other character, preferred solely because more lucrative;

whilst a third set, availing themselves of the belief which the Turks entertain of the hereditary nature of scientific and medical abilities, exalt themselves on the reputation of a father or uncle, and, despising the other subsidiary arts of advancement, start at once into authority and eminence. Nor is their presumption unrewarded or dishonoured. Any of these aiming at particular distinction may obtain, on reasonable terms, from the Hakim Bashi, a form of permission acknowledging their right to practice.

The second class, forming the great majority of those at Constantinople, consists of Greeks who have passed a year or two at the university of Padua or Pisa. This circumstance, which gives a more formal authority to their pretensions, is further used to force their practice by a display of technical terms and uncomprehended doctrines. But as their residence abroad is too limited to insure those acquirements in medicine, which depend more on study and experience than endowments of genius, it is natural to suppose that these students must, for the most part, enter on practice with little less acquired knowledge, and infinitely more dangerous hardihood, than the less favoured tribe of Dragomans and druggists; yet it seldom happens that these, by perseverance and the use of certain arts of chicanery, elsewhere reprobated, but which here pass current without opprobrium, fail to gain independence and even reputation.* It would be not less invidious than unprofitable to disclose the minute details of the practice pursued by the one and the other branch of these men. Suffice it to say, that in acute cases the system of Broussais, as prescribing blood-letting, diet and simple remedies, has been happily pretty generally adopted, to the exclusion of drugs, the efficacy of which, under the most prudent administration, is doubtful, and which in this climate, in the hands of ignorant adventurers, would be useless; and that in chronic cases patient and doctor are alike satisfied to alleviate urgent symptoms, and trust to circumstances to decide the result. A novel and most despicable feature of this routine should not, however, be unnoticed, namely, the avowed alliances, offensive and defensive, established betwixt bands of these individuals whose twofold object is to support each other's merits and doctrines at their chosen apothecary, and to approve and ratify each other's practice in the presence of the patient at consultation; and secondly, to oppose all collision of science with quackery, by attacking, with the vilest subterfuges, the character and skill of the regular practitioner. Woe to the European who, on his arrival, does not know how to conciliate their favours, or who dares question their pretensions; for so great is their ascendancy over Turks, Armenians, and Greeks, by knowledge of their language and caprices, that these, when they consult a Frank, seldom or never follow his prescription without subsequently having their value determined by the Jew or Greek in ordinary attendance; and therefore it is that the physician must, for a season, condescend to applaud the discernment, and sanction the acts of a person whose knavery would overrule his science. The habit of the natives to ascertain privately the particular opinion of one or more of those in consultation, after the general decision has been given, facilitates, if it does not constitute, this practice, to the advantage of the intrigues of one party, and detriment of honest principles of the other.

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* It would be insulting the good sense of many Greeks of Constantinople, who by a regular course of study, and by their talents and character, have deservedly obtained a well merited distinction, to offer any assurance that this criticism cannot reflect on them.

The practice of surgery is considered by the people a department of science very distinct from, and inconsistent with, the dignity of the physician; and although able surgeons have, by their skill and respectability, distinguished themselves, and been honoured by the Turks as general practitioners, yet, until the present day, the prejudice against the custom is such, that few Europeans profess to be Gerats, although, with two or three exceptions, all would operate. Surgery has, therefore, from this imaginary opprobrium, been abandoned to certain adventurers, and has fallen into disrepute. The probable cause of this disparagement may be ascribed to the ignorance of anatomy among native practitioners, and their consequent necessity to appear superior to an art, the dexterity and success of which may be appreciated by the commonest capacity.* Capital operations are unknown at Constantinople, partly from this cause, and partly from the few accidents occurring in civil life; and it is a notorious fact, that in the last war there were not five surgeons capable of performing the most common operations on the field of battle attached to the Ottoman army. Particular branches of the art, as bone-setting, hernia, and eye operations, are exercised by Persians and Moreats, with a success and celebrity similar to the like people with us. Midwifery is almost exclusively practised by Jewish and Turkish women; and it is worthy of remark, that the obstetric art forms a very small portion of their adroitness or employment. All pretend to possess, and some have become famous and wealthy by their pretensions, certain means, not only to obviate sterility, but also to produce abortion by administration of drugs,—a practice avowedly tolerated and frequently resorted to by Turkish females, both from their dislike to frequent pregnancy, and from command of their lords, when their harem threatens to become too numerous.

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* The abandonment of surgery in the capital cannot be better illustrated than by the fact, that a successful case of formation of the nose from the integuments of the forehead has acquired to the writer more fame and applause than the tying the subclavian would have done. The Sultan himself and his ministers sent to express their interest in the case, and had the man subsequently exhibited in full *divan*.

These facts, without further comment, exemplify the degradation of medicine at Constantinople; and the character might, with equal justice, be extended to the empire at large. Our regret and indignation are, however, somewhat assuaged in referring to the conduct and character of several European and Greek medical men here. With equal truth and pleasure we bear testimony to the integrity of those whose principles and talents redeem in some measure the honour of their profession, and recover a portion of that distinction and credit conferred on it in Europe. Without fear of being accused of national prejudices, it may be said, that Englishmen have always enjoyed a flattering station amongst the most distinguished practitioners of the place; and, at the present day, the humanity and skill of my friends, Dr M'Guffog and Dr M'Carthy, are rewarded by the confidence of the higher ranks of the empire, and a reputation amongst every denomination of people, perhaps never surpassed at Constantinople.

From these disagreeable details, sketched with as much leniency as fidelity permits, we turn, with gratification, to the improved prospects of medical instruction offered by the institution lately established at Constantinople.

We have seen the inefficiency of the former means of attaining this object, and its consequent evils; and we are therefore disposed to receive with satisfaction, and judge favourably of the endeavour, with all its defects, to bestow on this department a portion of those ameliorations developed in the scheme of military organization and general instruction planned and prosecuted by the Sultan. The groans and murmurs of the wounded of the last war have touched the sensibility of the divan, and shown the Moslem legislators the great necessity of yielding to the calls of humanity, and, by this establishment, of remedying the terribly augmented destruction of their troops from absence of surgeons. This medical school, at the expence of Government, was opened three years ago. It is under the direct control of the Hakim Bashi, whose talents and liberality fully qualify him for this important charge; and by whom teachers are appointed, and students selected. It is at present composed of four professors, two for the Arabic and French languages, and two for medicine and surgery, and 140 scholars from fourteen to thirty years of age, divided into these several classes. By an ill-advised economy, the salary of the first is too small to encourage any competition of respectable talents for the appointment; and, where the honour and influence thereto attached is questionable, this circumstance must affect the interests and utility of the seminary. But, on the other hand, the pay, rations, and prospects held out to the students have excited a great eagerness on the part of the better classes of the people to insure their children these advantages.

The first period of this course of education is spent in obtaining a competent knowledge of the Arabic and French languages, the object and utility of which are alike conspicuous. The second is devoted to medicine; but unfortunately the means of imparting or acquiring this knowledge are so imperfect that it is quite impossible for teachers, however talented, zealous not to compromise themselves, or for pupils, whatever their capabilities and application, not to be disappointed by the event. From the limited time of study, the non-appointment of additional lectures has not yet been so severely felt as the want of those means of practical instruction which are indispensable to the acquisition of useful medical knowledge, namely, anatomical demonstrations, chemical experiments, and hospital attendance. The latter defect was most manifest on my first visit to the institution, on witnessing the eagerness of the young men to impress a favourable opinion of their application, and the interest and pride with which they listened to inquiries and remarks on showing their manuals of anatomy, physiology, and materia medica, a new preparation of the blood-vessels of the head, some plates, and a skeleton, as if these, their library and museum, evinced and insured their acquirements, and unfolded the mysteries of medical science.

The lecture attended fully exemplified this. The person was surrounded by students in an oriental posture, with a French epitome of anatomy in their hands, from which he was labouring to explain, by translation and reference to a skeleton, one of the most intricate points of surgical anatomy, that of the shoulder-joint. The result of his utmost exertions was to store the learner's memory with technical words, of the import of which they could form no precise or useful application.

This pitiable abuse of time, industry, and genius, on the part of teacher and scholar, must be speedily redressed in the system of education, or experience will effectually support the malevolent ridicule of those opponents, whose selfish motives desire its failure; for, besides the positive harm done by teaching nothing, its continuance on the same plan must destroy all habit of reflection or reasoning, and implant false comprehensions of the extent and importance of the study.

These faults, into the origin of which it is useless to inquire, but which are in part inseparable from a system of instruction so little understood and appreciated in Turkey, may be alleviated, if not entirely removed, by the simple authority of the head of the institution, without encroaching on the prejudices of the people, or doctrines of the Koran. The only point which has entered into dicussion is that of dissections. My attention has been directed to this question by inquiries amongst native practitioners, people of the church, and others capable of knowing; and their uniform answer is, that there exists no law of the prophet, nor decision of his commentators, against anatomical demonstration, and, therefore, that a decree from the Mufti, or order from the Hakim Bashi, would be sufficient to authorize their performance. It is already fully sanctioned by the precedent of the school of medicine near Cairo. My friend Dr Clot, who has done himself distinguished honour by that establishment, had influence to obtain from the Pasha an unqualified order to employ the dead of the military hospital for this object. No moment can be more propitious than the present for establishing the custom, when the marvellous changes affecting the manners and opinions of the people, and acts of the government, hinder too severe examination of particular measures and prepare men's minds for every innovation. It is, moreover, believed that the spirit of the Turkish religion, which is stript of much of its former fanaticism and intolerance, would be easily reconciled to the matter, and would overcome any scruples which the Dervisch or Iman might oppose to this concession.

It must, however, be confessed, that there is no improvement which does not offer difficulties almost insuperable, even proceeding from those for whose benefit such changes would be made; and if the study of anatomy be too much in advance of the progress of general knowledge in Turkey, the only alternative of the government is to adopt the project already contemplated by the Hakim Bashi, namely, to send, at the public cost, a hundred or more clever young men of good families into England and France, where, by intercourse with polished society, they may be emancipated from the stubborn thraldom of native prejudices, and, by a university education, be qualified on their return to become teachers of the various branches of medical education, and wise and influential promoters of the political scheme of instruction. The most conversant with the character and views of Mahmoud suppose that he may be induced to support this design liberally, from being devoted to the project of enlightening his people, and forwarding them in that career of civilization, in which the rapid progress of his most redoubtable neighbours, contrasted with his own backwardness, has brought so grievous calamities on his empire.

To conclude by reverting to the schools, the establishment, though very imperfect, deserves our warmest commendation, since, co-operating with its contemporary in Egypt, it must effect a beneficial change on the Turkish state of medicine. The latter possesses the superior advantages of numerous well-informed instructors, of hospital practice, necroscopic inspections and demonstrations; but must yield to the former in the intelligence, application and capabilities of the students, which qualities, if rightly encouraged, would expand themselves to the honour of the teachers and founders of the school. The experience of its success has convinced the most sceptical of its expediency; and although its immediate confinement to the army be too limited, and its course of instruction hitherto too contracted to afford a speedy marked result, yet its very existence, and the satisfaction given by the pupils, encourage the hope that, under the enlightened care of the Hakim Bashi, a useful system of medical instruction will be established, and diffuse its benefits over countries in which ignorance, superstition, and empiricism have hitherto exercised an unlimited sway.

CONSTANTINOPLE, *June* 1830.

ART. II.—*Brief Sketch of the Fever which prevailed at Gibraltar in the Autumn of* 1828; *together with Observations on the Answers of Sir William Pym to Queries from the Royal Medico-Chirurgical Society of Cadiz, addressed to him on the origin and nature of that Disease.* By T. SMITH, M. D. Surgeon, 23d Regiment, or Royal Welsh Fusileers.

PASSING through Cadiz a few months ago, I met with a series of questions proposed by the Royal Medico-Chirurgical Society of that city to Sir William Pym, superintendant-general of quarantine in Great Britain, together with his answers relative to the epidemic yellow fever which raged in this garrison in the autumn of 1828; and, although I have hitherto taken no active part in the discussions on this subject, I have now determined to offer a few remarks on the statements made by Sir William Pym, as it appears to me many of them are founded in error; and may mislead, not only our neighbours the Spaniards, but the public in general, on the origin and nature of this malignant disease.

It will be recollected Sir William Pym follows Dr Chisholm and Sir Gilbert Blane, in believing that there is a form of yellow fever *sui* *generis;* different from that which ordinarily appears in the West Indies and North America, in being highly contagious,—affecting the same individual but once, like small pox or measles,—and never appearing in Europe, except when imported;—and from its having been originally imported, as Sir William Pym maintains, from Bulam, on the coast of Africa—he calls it the Bulam fever.

How far this opinion regarding the Bulam fever may be correct, I am not prepared to say. All I shall attempt at present will be to show that the facts connected with the history of our late epidemic do not warrant Sir William's deductions.

Before entering on the consideration of Sir William's answers, I shall give a brief sketch of the rise and progress of this fever.

The first account we have of malignant or yellow fever having visited Gibraltar is in 1804, when it carried off nearly 6000 of the army and inhabitants. It had visited Cadiz and the other neighbouring towns several times before this period; and the circumstance of its only appearing in these places periodically, engendered a belief that it arose from some foreign source, and was analogous with that disease which was contended by Dr Chisholm to have been imported into the West Indies from the coast of Africa in the year 1793. This was also the most generally received opinion in Gibraltar; and in 1804, 1810, 1813, 1814, and 1828, when the fever appeared here, it was attempted, by the supporters of this doctrine, to connect its origin with the arrival of vessels from the West Indies or North America; or, if this failed, to endeavour to trace a communication between the garrison and Cadiz, Malaga, Carthagena, or other towns in the neighbourhood, where the same disease was prevailing at the time. The circumstance, however, of not being always able to do this, as in 1814, when it could be traced to no foreign source, led many people to doubt, and ultimately to deny, the accuracy of this doctrine; and, at the breaking out of the late epidemic, many advocated the local origin of the disease and its non-contagious properties. To this conclusion they came, chiefly from the following reasons; namely, that, in former epidemics, the disease had never been satisfactorily traced to a foreign source, nor had its progress been marked by any of the ordinary laws of contagion; that it had always appeared about the same season of the year among the lower orders, particularly where the population was dense, the houses badly ventilated, and where due attention to personal and domestic cleanliness had been wanting, &c. &c.; and that it was not contagious, they inferred from its never having been carried from one locality to another by persons or other means; from its not having spread either at Europa Flats, the Neutral-Ground, Wind-Mill Hill, or in the Bay, amongst the shipping, although free communication between the town and these places were permitted; and, lastly, from similar facts having been repeatedly observed in other countries, where yellow fever appears epidemically.

Public attention was first called to the existence of a disease of a suspicious character, by an official letter from Dr Hennen, of the 29th August, to the Lieutenant-Governor, in which he states, that five cases had been reported to him occurring in No. 24 District, one of the most crowded and filthy in the town, at the back of Hargrave's Parade. The Doctor designates these, cases of the "Bilious Autumnal Remittent Fever;" but states, that some of them had assumed all the appearances of yellow fever, and that one had proved fatal the day before; that the family of a Mr Martin in the same district (four in number,) had been attacked with a similar disease; and that the servant maid of this family had died on the 28th, after five days illness. On the 31st, two other cases were reported in the same district in children; one in Victualling-office Lane, and a death in the Civil Hospital; and on the 2d September, Dr Hennen reported to the Lieutenant-Governor, that a coachman of a Mr Duguid died on the 1st of the month of fever in 24 District; that one boy, and one girl, from the same district had been admitted into the Civil Hospital, in addition to three other cases reported to him,—one by the inspector of the district,—another, the servant of Mr Hasson of the Civil Prison, by Mr Fraser,—and the third by the medical officer in charge of the district. And, following Dr Hennen in his official account of the progress of the fever to Sir George Don, we find that two deaths occurred on the 2d in 24 District; one a young Jewess, under charge of a civil practitioner, and among those first reported on the 29th ultimo; and another man that was taken ill in a miserable hovel, with a sheet-iron roof. Six fresh cases were at the same time reported; five in 24 District, and one in George's Lane, and three of the five occurred in the house in which Mr Duguid's coachman died; "and it is not a little remarkable," says Dr Hennen in the same letter, "that no fewer than nine cases have, within these ten days, occurred in this very house. Should any more occur, I am strongly disposed to submit to your Excellency, whether it may not be a proper measure to order it to be evacuated until it has undergone a proper purification." On the 4th of September, Dr Hennen wrote to the army medical board, that twenty-eight cases had occurred in all, and five deaths; and on the fifth he reported its appearance among the military to the Lieutenant-Governor, and says, "that so many cases of a fever of a very serious nature have appeared in the barracks of the Sappers and Miners on Hargrave's Parade, which I would observe to your Excellency is in the line of the drains coming from District No. 24, that I feel myself called upon to submit the propriety of immediately encamping that corps, and totally evacuating the barracks." One case occurred in the 12th Regiment on the 2d of September, and the man died on the 5th. The barracks of this corps are situated upon a line of drains, one of which burst in the centre of the buildings, and emitted most offensive smells during the summer months. It may be remarked, however, that this man was in the habit of visiting a female friend in 24 District, and must thus have been exposed to any insalubrious local influence. It would be needless to follow the progress of the fever farther to individual cases.

With regard to the locality of the district, No. 24, in which so many cases of this fatal malady first appeared, as it were simultaneously, Dr Hennen makes the following observations in his letters of the 29th August to the Lieutenant-Governor, which have been published by Mr Fraser in the Medico-Chirurgical Review, No. 26, 1830. "The whole of these cases," says he, "as I am informed by Mr Wilson of the Civil Hospital, are in the line of a drain which comes from Mr Martin's house; and within a few yards of the doors of the tenements where the individuals have been taken ill, there is a large open grating, from which very offensive vapours are said to arise; and Mr Wilson states to me, it is hardly possible to be otherwise, as no fewer than four privies empty their contents into the drain." Again, in a letter of the same date, "after a most minute personal inspection of the whole neighbbourhood," Dr Hennen states, "in reference to my letter of this day's date, I have minutely inspected district, No. 24, in company with Mr Wilson of the Civil Hospital, Mr Woods, the medical officer attached to that district, and other staff-officers, and it is with much regret I have to state to your Excellency, that in almost every step I took in that district, I had reason for surprise, not that fever had broken out there, but that it had not extended farther; from whatever causes it may have proceeded, the pauper population is dense to a degree incredible, except to those who have seen it. In sheds without ventilation, without drainage, and generally composed of the slightest materials; in tiers of beds as close as in a crowded transport, numerous individuals sleep; they go out to their work at an early hour, and return at gun-fire, locking up their miserable places of nocturnal shelter during the day, and leaving them saturated with the steams of their bedding, their food, and the overflowing receptacles of their ordure."—"The detail," he adds "would be too disgusting to enter into; but I most respectfully submit to your Excellency the indispensable necessity of sweeping away the whole of these sheds, which I have every reason to suppose are unauthorized by the government." Dr Hennen caused the following notice to be inserted in the Gibraltar Chronicle at the breaking out of the epidemic.

"NOTICE.—It having been ascertained that several of the drains in private houses are choaked up, and are left uncovered, and in consequence emit vapours highly prejudicial to the health of the inhabitants, it is recommended that no time be lost in putting the drains in order, and procuring covers, which should be kept constantly on with a proper weight over them. Close covers should also be placed over all the privies, and a mixture of quicklime, charcoal dust, and water, should be poured into them, at least once a-day, and the utmost attention to cleanliness, and the frequent use of lime, especially in ground floors, and ill ventilated apartments, are most particularly enjoined. * * * * * The inspectors of districts should be specially charged with the execution of this order."—See Gibraltar Chronicle, 7th September 1828.

In fact, the whole of Dr Hennen's prophylactic measures were founded on the belief of the local origin of the fever. Drains by his order were opened, cleaned and washed; filth and nuisance of every kind removed; houses ventilated, white-washed, and otherways purified; the population as much as possible thinned. The troops were encamped; the inhabitants were removed from No. 24 District; and a commission was appointed by his Excellency the Lieutenant-Governor, at Dr Hennen's suggestion, to inspect the different districts of the town. The proceedings of this commission corroborate the Doctor in this view of the question, as may be seen from the following correspondence.

" SIR, *Gibraltar, 1st September* 1828.

We have the honour to report for your Excellency's information, that we this day inspected 24 district, and several districts adjoining, in execution of your Excellency's communication to us on the 30th August, and we have found that the understated sheds have been erected without the consent of government, and are decidedly inimical to the health of any persons who may inhabit them, viz. 1*st*, Wilson's Buildings, on the road to Flat Bastion, two wooden sheds,—2*dly*, Morella's Buildings, one wooden shed,—3*dly*, Mr Haire's house, two wooden sheds, in which two very serious cases of fever occurred,—4*th*, Benzurbra's Passage, one tenement.

" In addition to these sheds, which we recommend to be immediately razed to the ground, we beg to call your Excellency's attention to the following tenements, which we found in a state that we conceive unfit for the habitation of man, without undergoing several alterations, viz. Falacher's house, Victualling-office Lane, Benzaqueen's house, in South Port Street, and Bensurbra's, behind the Town Range Barracks. (Signed) JOHN DUFFIELD, Chairman; J. HENNEN, M. D.; HUGH FRASER, Surgeon; ANTHY. TERRY; A. CAMPBELL, Town Adjt.

" To His Excellency General Sir GEORGE DON, &c. &c. &c.

" SIR, *Gibraltar, 2d September* 1828.

" We have the honour to report to your Excellency, that, in pursuance of your orders, we this day proceeded to a farther inspection, extending from the vicinity of No. 24 district to the Moorish Castle, and we have to report the following buildings, which have been erected without permission of government, and which we conceive it indispensable to the public health should be as early as convenient razed to the ground.

" DISTRICT, No. 25.—Eveleigh's ground,—Two sheds covered with tarpauling; near Arengo's Gully, two sheds additional,—Eveleigh's buildings, one shed in the yard.—Oliveras' grounds, six sheds.—Cavallero's platform, one wooden shed.—Minchen's house, four out-buildings.—Mr Bracbridge's stable-yard, one shed.

"In addition to these sheds which we recommend to be destroyed, there are three other sheds, and an old wall in Cavallero's and Arengo's buildings, which we conceive ought to be new modelled.

"In the course of our inspection, we were struck at every step we took with the density of the population; and although this is an evil which demands prompt and rigorous measures for removal, it was gratifying to us to see that in every inhabited house the occupants appeared to be actively employed in cleansing and lime-washing,—a measure which we would most strongly recommend to your Excellency to enforce by a police regulation.

"We observed several tenements belonging to the crown empty, and we would beg leave to suggest to your Excellency that, as by the razure of some of the sheds mentioned in the first part of this letter, a few individuals will be left entirely without shelter, they may be permitted to reside in the crown houses. The rest of these houses should be completely secured from encroachment, as we found one of them broken open, and in a state of the most disgusting filth.

"Among the persons who will be deprived of shelter by the razure of these sheds, are two soldiers' wives, recently arrived in this garrison on their route to Corfu. We are induced to notice these individuals the more particularly, as it affords us the opportunity of recommending that all persons of this description should, with as much speed as possible, be removed from the garrison.

"We have, in the course of our inspection, had constant reference to the plans deposited at the crown land-office, every encroachment upon which, will, we trust, be removed; but we cannot help recommending to your Excellency to give directions that the opening of all drains, cess-pools, and privies, be accurately covered up, so as to prevent the emanation of all vapours from them, and to remain so covered, except when opened to admit of their being cleansed, and for ordinary purposes.

"In the course of our inspection this day, we found no case of disease in the houses; and we beg leave to supply an omission we made in our report of yesterday, by stating that we detected one case of fever in a girl in district 24, whom we ordered into hospital immediately. She is now doing well.—We have the honour, &c. &c. &c. (Signed) JOHN DUFFIELD, Chairmain; J. HENNEN, M. D.; H. FRASER, Surgeon; A. TERRY.

"*His Excellency, General Sir* GEO. DON, *&c. &c.*"

I now return to the progress of the fever amongst the military; together with its origin in the south, and Catalan Bay. I have already stated that the Sappers and Miners became early victims, and also a sergeant of the Twelfth Regiment, and that the barracks of both these corps were situated upon a line of drains leading from the district infected. They were encamped on the 5th September; and the same day the Spaniards encompassed the place by a cordon. On the 3d, the Governor of Algesiras sent over a deputation of medical officers to inquire into the nature of the fever It is remarkable that these gentlemen did not carry the disease into Spain. They visited every case in the garrison, and could not fail to have had their clothes saturated with the contagious principle, or themselves contaminated, had any contagious properties existed. The Sappers were at first encamped at Poco-Roco-Cave, and the Twelfth on the Neutral Ground. The Sappers were soon removed from Poco-Roco, as the fever continued to prevail among them, this place being within the circle of epidemic influence; but in the Twelfth not a case appeared after they were encamped (except amongst the orderlies and servants, who remained in town,) until the 23d September, when the regiment commenced again night duties in town, from which it had been relieved on going into camp, and then the fever became general in the corps. Mr Amiel, surgeon of the regiment, states that, out of ninety-two women, and one hundred and ninety children, who were never allowed to pass Bay Side, (the entrance into the garrison,) not one had the fever, though several of them slept in the same beds with their husbands labouring under the epidemic, and continued to use the same bedding.

The first fatal case in the 23d Regiment, which was quartered in Rosea barracks, occurred on the 16th of September The man was admitted on the 13th from town. He had been employed as a blacksmith in the commissariat yard, where the mortality was afterwards very great. This yard is situated between the barracks of the 12th Regiment and the Sappers and Miners, on the line of drains from 24th district.

The regiment was encamped at Europa Flats on the 17th, and the first cases after this are two non-commissioned officers, one on the 24th, the other on the 30th—both had been employed at the convent in town. After this the disease became more general; but was confined exclusively to those whose business led them to town. I am not aware of a single case occurring in the 23d Regiment where the man had never left camp; and the immunity enjoyed by the women and children of the corps was universal. One man is stated by the commanding officer of the regiment, in his evidence before the board of commission,* to have been attacked without his ever leaving camp, but it was not fever with which he was admitted into hospital, but dysentery.

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* To enable the reader to understand the nature of the different commissions alluded to in this paper, namely, the "Board of Commission," and the "Anglo-French Commission," it is necessary to observe, that the colonial secretary (General Sir George Murray) caused to be instituted a Board to investigate into the origin of the epidemic fever, to be composed of the following members, and to be designated the "Board of Commissioners."

President, His Excellency, General Sir GEORGE DON.
MEMBERS.
Colonel CHAPMAN, Civil Secretary.
Judge HOWELL.
W. SWEETLAND, Esq. Captain of the Port.
Lieut.-Col. FALLA, Town Major.
Drs PYM and BROADFOOT.

Sir George Don delegated the duty of president to Sir William Pym, who appointed Dr Barry to be the secretary.

At the time the first cases of fever were admitted into hospital, there were men with other diseases in the same wards, and I never observed any mischief to result, although no precautionary measures were adopted. For example, from the 7th September, when the fever was declared to be in the garrison, to the 15th of October, a period of five weeks, thirty-six cases of other diseases were received promiscuously into the hospital with the fever patients, and out of these only eleven were attacked with the disease. One man was seventy-five days before he was attacked; and another with a fractured leg, although thirty-four days in hospital, never caught the disease. The hospital servants escaped also until the disease had become general in the district where the hospital is situated; and, up to the time of Dr Hennen's death, on the 3d November, there was no prohibition to a free communication between the hospitals and the camp. The convalescents were discharged directly to their regiments without their *kits* or wearing apparel being submitted to any process of purification; and I myself had daily communication with the camp. The officers also, who confined themselves to the camp, escaped; although some of them accompanied their sick companions to hospital, rendering them every kind office, and sitting with them in the same waggon which conveyed them to hospital. The other regiments, 42d, 43d, 73d, and 94th, were attacked much about the same time, and were severally encamped, the first two on the Neutral Ground; the 73d, and 94th, on Europa Flats, and Wind-Mill Hill.

In the Civil Hospital, such was the pressure for admission into that institution, as to render it necessary to relieve the wards the moment a patient passed the *acme* of disease; accordingly, convalescents were discharged in all stages, many of them having blood exuding from their gums; and to some very poor people the soiled blankets under which they had passed the disease were given to them. The majority of these people went to the neutral ground, where they recovered rapidly, and where they slept with their families as usual, and mixing with whom they pleased. Where, then, existed the bugbear contagion?

It was observed that the cases at the beginning of the epidemic were more severe, as well as more numerous, in the 43d than in the other corps, and this was attributed to the nature of the duties of that regiment. Particular stations, as North and South Flat Bastions, South Port, and the convent guards, were found to give rise to more concentrated forms of the fever

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At the breaking out of the epidemic, the French government appointed three physicians to proceed to Gibraltar to inquire into the causes of the disease, namely, Drs Chervin, Louis, and Trousseau. On the arrival of these gentlemen, Dr Barry and Mr Peter Wilson of the Civil Hospital were associated with them, for the purpose of forming a board to be denominated the "Anglo-French Commission."

than others; and the soldiers were attacked in greater number on these than on the other guards; and it is worthy of remark, that, with one exception, these posts were on the line, or immediate vicinity of the drains leading from 24th district.

The *line wall*, skirting the sea-beach, was also discovered to be a very mortal station. Few sentries planted there, if any, escaped an attack; and Colonel Payne, commanding the artillery, who remained in the town from the commencement of the fever until his death in the month of December, lost his life by visiting this locality at night. The known unhealthiness of this station caused many of the sentries to be withdrawn from it at night, and the consequence was, some depredations were committed on the great guns, and the Colonel, in his zeal to detect the offenders, perambulated this district for three successive nights, and on the third was suddenly attacked, and expired on the fourth day of his illness.

The fact already noticed of persons enjoying immunity who strictly confined themselves to the different camps, as well as that of the disease never spreading in these places, were confirmed by daily observation, in all the regiments in the garrison.

The period of the first appearance of the fever in the south is involved in some doubt; but it is certain it did not become general there for nearly a month after it had been prevalent in the town, and which could not have happened had the disease been contagious, as there was a constant communication between these two places. The Naval Hospital is situated in this district, and the repeated intercourse necessarily kept up between the servants of that establishment and the neighbourhood could not fail to show its bad effects on the health of the inhabitants earlier, had contagion existed. The first case which occurred was in Dannior's Buildings, on the 31st August, in a woman of the name of Domine, who went to town on that day to visit a sick friend, and suckled the child of that friend, Bernarda Riva, who lived in Caille Comedia, and on returning home in the evening was taken ill when passing South Port Gate, the original focus of the malady, and for three or four days laboured under a very severe attack of fever; yet no one else in the same building was taken ill till nearly a month afterwards, when the disease had become general, and after twenty-six individuals had died in the same or adjoining houses to the place where this poor woman lived. The next case was a young lad of the name of Acres, whose mother lived near the bridge leading to the Naval Hospital, in a small confined wooden shed, close to a gully, through which runs a drain, and which emitted very offensive vapours during the summer, This lad had been living for some time before he was taken ill in No. 24 district, and was three or four days in his mother's house before he was sent to the Civil Hospital on the 5th September. His mother had had the fever during a former epidemic, but a sister who lived in the same house, and slept close to her brother in the same room, was not taken ill before the 22d of the month, when she was sent to the Civil Hospital, and died on the 28th. Two daughters of a Widow Moir, in the same neighbourhood, were taken ill, one of whom died. From this period the disease became general in the district.

I shall now advert to the origin and progress of the distemper in Catalan Bay, a small fishing village situated on the eastern side of the Rock. The first person attacked was a poor old man, named Andrea Andre, who dwelt in a large cave or excavation of the Rock, directly in rear of the clump of buildings where the quarter of the officer commanding the detachment is situated. This individual possessed a donkey, and earned his livelihood by carrying water from the wells on the Neutral Ground into the town, so that he was in the daily habit of entering the garrison, and returning every evening to the Bay. On Friday the 19th September, he was reported to Captain Jenkins, the officer in charge, to be ill with fever, and was in consequence sent to the Lazaretto the following morning, where he died on Monday the 26th. The second person taken ill was the servant of Captain Jenkins, a private soldier of the 12th Regiment, named Daniel M'Curry. He was attacked on Monday the 22d September, and sent to the Military Hospital on the 24th, where he recovered. Upon M'Curry's being sent to Hospital, Captain Jenkins took into his service another soldier of the detachment named Farrell, who sickened on Thursday the 25th September. This man slept in barracks, and was the first of the detachment who fell ill. From this date (25th September) until the Saturday following (27th September) a serjeant and three or four men were attacked and sent into the Military Hospital.

In the civil population, the first case that took place after the sickening of Andre on the 19th September, was a girl twelve years of age, named Margaret Martin, who resided in Captain Jenkins's house. This girl was servant-maid to a lady who had come from town on a visit to Mrs Jenkins. She had left the garrison on the 6th or 7th September, and was attacked with the epidemic on the 27th, and immediately removed into the Civil Hospital, where she recovered. The second case was a little boy, who resided next door to Captain Jenkins. He was taken ill about the 25th September. The third occurred in the son of Ferecono Beso, next door neighbour to Andre; and the family of Domingo Colias, who dwelt close to the house of this man, fell ill on the 4th of October. After this it is difficult to trace, with precision, individual cases; for several families sickened about this period, who acknowledged before the Anglo-French Commission, that they had passed the disease in their own houses without its being known, fearing they might be sent to the Lazaretto. Captain Jenkins, in his examination before the Board of Commission, states, "that twenty-one civilians in all were sent out of the village into the Lazaretto and Camp of Observation, at the order of the attending physician, and out of which ten died."

The number of families who concealed their illness, so far as I have been able to ascertain, amounts to five; and, as they all had children, twenty persons may have passed the disease in their own houses;—it is remarkable that not one of these died. Including the military, therefore, the disease attacked in this small village about fifty persons, of which ten civilians died and one soldier. Captain Jenkins states the population to have consisted at the time of 576 civilians, men, women, and children, and from fourteen to twenty soldiers; but I have heard persons of great local information say, that the number of civilians is underrated.

" It has been stated that the disease was introduced into the Bay by Andrea Andre, who, it is asserted, infected Daniel M'Curry. The latter, in his examination before the Board of Commission, states, " That, on Friday the 19th September, when Andre returned to the Bay, he had a bundle on the top of his kegs on the Borrico. I (M'Curry) was passing towards Captain Jenkins, when Andrea asked me to assist him to take off the bundle; I did so, and passed on to my master's quarters. I could not tell that the bundle was filled with clothes, but it appeared to contain linen."

On being asked by the Board if he had any other communication with Andrea, he replied, " That he had two shirts washing by Andrea's wife, which he went to fetch; that this was some days before he helped Andrea off with the bundle, and that Andrea had no appearance of being sick at that time." Such is the sum and substance on which the opinion of the introduction of the fever into this village is founded. Had Andrea laboured under the true eastern plague, the communication which took place between these two individuals was hardly sufficient to have affected M'Curry; but the truth is, we must look for some other cause to account for M'Curry's fever, as Andrea was not capable of infecting the inmates of his own family, with whom he must have had close communication, his wife being alone attacked out of seven persons; and that the nature of the locality was the chief, if not the sole agent in propagating the disease, I think, will not be denied, when it is recollected the fever was confined to this part of the village, which was in a very filthy state.

After this brief enumeration of the leading facts illustrative of the origin and progress of our late epidemic fever, I ought perhaps to say something of the symptoms by which it was characterized, as well as of the different modes of treatment; but as I shall have occasion to notice these when commenting on the answers, I shall for the present defer this part of the subject; suffice it to remark, that this truly formidable disease was acknowledged by all parties to be identical with the black vomit fever of the West Indies and North America, and of the same nature with that which had at former periods depopulated this garrison.

Of the causes of the malady, I am unable to speak with any degree of certainty. That it was not imported, I think every candid man will admit who has deliberately weighed the evidence given on the subject before the Board of Commission, and the facts I have, and shall hereafter state; and were it not that my friends, Messrs Fraser and Wilson, have already so fully discussed these topics, I should have been induced to enter more at length into the question here. In fact, every endeavour to establish the importation doctrine has failed; and both the Colonial secretary, Sir George Murray, and Sir James M'Grigor, director-general of the army medical department, I have heard, are convinced there is not the slightest grounds for such a belief; but, on the contrary, that there is every reason to suppose the disease owed its origin to causes within the walls of the garrison.

What those causes are is not easily determined. By some the disease was referred to a vitiated atmosphere, a dense population, close and ill-ventilated apartments; a bad state of the drains, exhalations from the privies, &c.; while others were of opinion, that it owed its origin to the soil, or to emanations resulting from the decomposition of animal and vegetable matter, either singly or combined; but how far either the one or the other of these are correct I shall advance no opinion. It was evident, however, there was a febrilizing influence present in our atmosphere the whole of the year 1828, as was manifest from its effects, for scarcely a month passed without cases of sporadic yellow fever occurring. As early as the 11th of January, a Jew, named Kian Bensimol, was admitted into the Civil Hospital, with well-marked symptoms of this disease, and died on the fifth day, vomiting great quantities of black matter. On the 13th of March, Solomon Anachony, a Jew also, was attacked with great severity, but recovered; and much about the same time, Mr Haden of the commissariat department fell a victim to the disease. On the 14th July, the orderly at the Civil Hospital had a smart attack, accompanied with yellow skin, although he had had fever during the epidemic in 1813; and on the 16th of the same month, another Jew, named Maclief Bennues, was admitted into the Civil Hospital, and expired on the eleventh day, with yellow skin, and vomiting of matter of the colour of coffee grounds. Besides these admissions into hospital, other cases occurred in the garrison. Abraham Bensimon, for example, died with "black vomit" in Irish Town, on the 3d May, after a few days illness; and on the 3d or 4th of August, Maria Pescadina, servant to Mr Fraser, was attacked, recovered partially from this attack, and relapsed on the 27th of the same month. Mr Hasson, keeper of the Civil Prison, was severely attacked on the 15th of this month, and his servant on the 17th; and at an early period of the season febrile attacks were so numerous in the 23d Fusileers, as to call the attention both of the commanding officer and myself to the subject; and an official communication was made, referring the cause of our unusual sickness to the bad state of two drains, which were in the immediate vicinity of the barracks, which were choaked up with a large quantity of soil accumulated at their mouths, emitting very offensive effluvia. There was nothing, however, remarkable, or any physical change appreciable by our senses in the season of 1828 from that of former years. The summer was cool. There was a prevalence of westerly winds; but bad smells were complained of by old residenters, some of whom predicted a sickly season from this circumstance alone; and I may here mention, that on cleaning out some of the drains, cart-loads of dead rats were found; and there was said to be an unusual accumulation of soil in the public privies, which I have heard had not been cleaned out for six years.

I shall now proceed to the consideration of the questions alluded to at page 12. A literal translation of them has not been attempted, but the sense has been invariably adhered to.

Translation of interrogatories from the Royal Medico-Chirurgical Academy of Cadiz, relative to the fever prevailing in Gibraltar, addressed to Sir William Pym, superintendant-general of quarantine of the United Kingdom of Great Britain, together with his answers.

Ques. 1.—What has been the origin of the fever which prevailed in Gibraltar? Has it developed itself spontaneously, or has it been imported? If the first, show or point out as far as possible the local and atmospheric causes which have generated it; and if the second, that is to say, if it has been imported, say by what vessel, from whence she came, what was her cargo, what length of passage, and what occurred remarkable during the voyage, what day she arrived in Gibraltar, how many days of quarantine; and lastly, if the persons, clothes, or other effects which came by her, had communication or contact with the first individual in the town; or with their families?

Ques. 2.—If it be not possible to answer the second part of the preceding question, say if, in the last summer previous to the appearance of the first cases of the prevailing fever in Gibraltar, any vessels had arrived in that port coming from the West India islands, or United States, which during their voyage have had any sick or deaths; of what diseases, what were their bills of health, and if, in consequence, they were put in quarantine, or sent to a lazaretto for the purpose of being disinfected?

Ans. 1 and 2.—There is not the most remote reason for supposing that the epidemic of Gibraltar originated from local causes. Without doubt it was introduced. A great number of vessels arrived from Havannah, St Domingo, and New Orleans; and the vessel which has justly been suspected of having introduced the disease, is the Swedish ship named Dygden, which arrived in this bay from Havannah on the 27th of June, of whose crew nine were attacked during the voyage, and two of them died. Her cargo consisted of sugar and logwood. The passage was forty-seven days. She was forty days in quarantine after her arrival. She was admitted to Pratique on the 6th of August, but the cargo had been removed twenty days before.

On the 10th August a boatman with his son, daughter, and a boy belonging to the boat, went on board a vessel in this bay, the name of which is unknown, because the father, son, and daughter, are dead. The son, a youth of 13, was attacked with the fever on the 11th August, the next day after being on board, and died on the 17th; his sister sickened on the 17th, and died on the 21st. The boy belonging to the boat was also taken ill on the 17th, and sent to the Civil Hospital on the 21st, and recovered.

The father died on the 7th September, his family was the first that suffered from the disease in Gibraltar.

From this time to the 2d September, it spread through 24 District, where this family resided. Then a nurse in the Civil Hospital, attending a patient from 24 District, fell sick, and at the same time a maid-servant of the surgeon of the Hospital, who resided in that establishment, was attacked with the disease.

Observation.—Here we have two questions embracing points of the greatest importance in the origin and nature of our Gibraltar yellow fever, and we have seen the manner in which Sir W. Pym has answered them. The opinions he has advanced I shall endeavour to show are at variance with facts. "There is not," says he, "the most remote reason for supposing that the epidemic of Gibraltar originated in local causes. Without doubt it was introduced." How Sir William Pym arrives at this conclusion I know not. This statement is contradicted by the history of the fever itself, by the history of former epidemics, the proceedings of the Board of Commission, the correspondence of Dr Hennen, and by the observation and experience of almost every unbiassed man in Gibraltar; —in short, it is an assertion perfectly gratuitous, and originating merely in the desire of the author to support a favourite hypothesis. The only plausible ground on which Sir William Pym rests his assertion of introduction is the Swedish ship Dygden, which has "justly been suspected," says he, "of having introduced the disease." This suspicion, however, is confined to Sir William Pym, and those who adopt his views; and to the opinion of these gentlemen I may justly oppose those of Judge Howell, Colonel Chapman, and Mr Sweetland. The first, who had no preconceived opinions to support, expresses himself in the following terms: "Upon a careful review of the proceedings before the Board, I am of opinion, that the evidence brought forward has totally failed to prove that the late epidemic disease was introduced from any foreign source, either by the Swedish ship Dygden, or by any other means; and I am farther of opinion that the late epidemic had its origin in Gibraltar." Colonel Chapman, civil secretary in this garrison, is equally decided. "Judging from the evidence produced before this Board, the manner in which it has been given, together with the description of persons who have been brought forward as witnesses, I am decidedly of opinion, that the attempt to prove the introduction of the disease, after months of previous inquiry by those who wish to prove it, have wholly failed."

The neutral opinion of Mr Sweetland, captain of the port, deserves attention also, as he, who was for many years known to have been a strenuous advocate of importation and contagion, has been candid enough to confess that he took his seat at the Board of Commission strongly prepossessed with the doctrines of contagion, and his belief in which he has now partly, if not wholly, abandoned. His opinion is as follows: "After the most attentive consideration of the evidence which has been brought forward, I have discovered nothing which has carried conviction to my mind as to the cause or origin of the late epidemic fever. On the one hand, it has not been shown that any of the causes stated in support of the doctrine of its being of domestic origin, existed in a higher degree in the year 1828 than in many preceding years, when the garrison was free from that disease; and on the other hand, no vessels have arrived during the last summer having the yellow fever on board, nor has that disease discovered itself among any of the shipping in the port. In the absence, therefore, of any proof on either side, I must decline hazarding an opinion on a subject which has hitherto baffled the researches of the most learned physicians of all countries.

Dr Hennen does not attach the least importance to the arrival of this ship. His letter to the Lieutenant Governor of the 2d August on this subject, is as follows:

" 757. *2d August* 1828.

"SIR,—I have the honour to acknowledge the receipt of your Excellency's letter of the 1st August and its enclosures, (herewith returned,) relative to the Swedish ship Dygden, and I beg to inform you that I have already minutely inspected the captain and crew, whom I found in perfect health, and I shall repeat my inspection before the expiration of their quarantine, on the 6th of the present month. In my letter of the 29th July, I mentioned, as the reason for putting this ship in quarantine for forty days, that two men died on the passage. It is now sixty-six clear days since the first man died, and sixty-one since the death of the last, and nothing like disease has since appeared, nor have I the most distant reason to apprehend danger to the public health from any circumstance connected with the Dygden.—(Signed)—J. HENNEN, M. D. Inspector of Hospitals."

The opinion of Sir William Pym given to the Board of Commission is couched in less positive terms than in this answer to the Cadiz Academy, for with all his predilection for importation, he is afraid, from the evidence before the Board, to fix it on any particular ship, but refers it to the shipping in general. His opinion is thus given: "From the very strong evidence before the Board, of the first persons attacked by the late epidemic fever having been connected with the shipping, I am of opinion that the disease was of foreign origin, and that neither local or atmospheric causes have had any share in its production, Gibraltar being one of the most healthy spots in Europe, no soldiers of the garrison having died of fever during the months of August, September, or October of 1827; and only nineteen soldiers having died from fever in the course of the five years preceding the year 1828, during the sickly months of August, September, and October."

After this statement, it might be unnecessary to say any thing farther of this ship; but as on her hinges the only attempt at importation, I shall state all the facts which have come to my knowledge connected with her voyage and stay in this bay. I have already quoted Dr Hennen's letter on the subject, by which it appears that no less than sixty-one days had elapsed without any thing like disease occurring at the time the vessel got Pratique, and Mr Sweetland, the captain of the port, in his memorandum on the same subject, confirms this opinion, and infers that the disease of which the two men died was not yellow fever, as it did not extend itself to any other of the crew. We have, besides these two gentlemen, the declaration of the captain of the vessel, embracing all the particulars connected with the voyage and arrival of the ship; and as this document is important, it is subjoined.

TRANSLATION.

"SIR,—Having since my arrival here heard various reports, by which it is pretended that the yellow fever was introduced last year at Gibraltar by the ship Dygden, under my command, it being at the same time imputed to me that I had made false declarations; that it was after not having been admitted at Malaga that I proceeded to Gibraltar; that the clothes of the seamen who died were sold there and smuggled on shore, &c. &c. all which are injurious to my character, I have considered it as my duty to state to you shortly, and as far as my memory will allow, what occurred on board the said ship in the Havannah, and on the passage, in order to clear myself from unfounded suspicion and aspersion, being ready, if required, to corroborate the same by oath.

"During my stay in Havannah none of the crew brought there were ill so as to require to keep in bed. A part having deserted there, I was under the necessity of hiring in their place six other men of different nations, of whom one being taken ill on the same day he came on board, was in consequence sent back again on shore. I sailed from Havannah on the 12th of May, with a crew of fifteen men, all in good health. A few days after two of the Swedish part of the crew complained of severe headachs, and pain in their limbs, which increasing they had to go to bed. Through sudorifics and purging medicine they got well in eight days, so as to be able to attend their duties. During that time five others had been taken ill of the same complaint, but recovered in a few days under similar treatment. A lapse of ten or twelve days followed, during which the whole of the crew were in perfect health; but upon getting into a higher latitude, I met with gales and rain, when the greater part of the crew suffered much from wet, and immediately after, those who had till then been well were taken ill, probably from cold produced by the weather, to which they had been exposed, yet the symptoms which appeared were the same as in the others. The youngest of them recovered in a short time, but the eldest two died, one after five, the other four days' illness, which took place on the 27th of May and 1st of June. The old clothes they had worn, together with hammocks and what was in them, was thrown into the sea with the bodies. Their best clothes, which I never once had seen them use during the voyage, were sold among the crew; but I cannot recollect that any of the articles were bought by any of the men paid off in Gibraltar; if so, they were at any rate very few, the greatest part of them being woollen clothes only suitable for the north. The sale was effected about three weeks previous to my arrival in Gibraltar, where I arrived on the 28th of June, without having communicated on the passage with any vessel, or touched at any port.

" After having made a true declaration to the pratique master, the ship was adjudged a quarantine of forty days, at the expiration of three-fourths of which time two quarantine guards were put on board, by whom it was ordered, that all the clothes, hammocks, boxes, and sails of the ship were to be ventilated, which was done twice, and the ship twice fumigated with sulphur. After having obtained pratique, five of that part of the crew which were shipped in Havannah were paid off, being as follows, viz. James Barron, William Whitman, Alexander Kanedy, D. Sinclair, John Brinkmore, who took with them the few articles of clothes that belonged to them, which, however, were of so little value that I suppose that they could neither have been sold nor found to be worth the expence of washing; but about this I have no knowledge; but in regard to myself and the rest of the crew, no clothes were sold or sent on shore, nor were any given to be washed until the ship had obtained pratique at Cadiz. In lieu of the seamen paid off in Gibraltar six others of different nations were shipped, and having accompanied the vessel to this place, performed in her here a quarantine of thirteen days, after which they left the ship, being all perfectly well. The rest of the crew were sent home in the Russian ship Helsingfoes, the Dygden having been sold, and arrived all in good health.—Without any more whatever to add, I have the honour to be, Sir, your most obedient servant,

Cadiz, 4th June 1829. (Signed) H. G. Gerle.
John Tarris, Esq.
Swedish and Norwegian Consul, Cadiz."

The captain, it will be seen, states, that the whole of his crew were taken ill during the voyage, (and not nine, as Sir William Pym tells us,)—that he embarked six fresh sailors at the Havannah—that the old or Swedish part of his crew were the first taken ill,—and on arriving at Gibraltar, after he had got pratique, that he discharged these foreigners with what clothes or bedding they had at the time—that shortly after leaving the Havannah on the 12th May, seven of a crew out of fifteen were seized with headachs and pains in the limbs, but by sudorifics and purgatives got well in a few days—that ten or twelve days after the rest of the crew sickened, but not till the ship had arrived in a higher latitude when the men were exposed to gales of wind and rain—and that the two oldest of them died. There is not a word, however, of fever, nor is there any alarm excited in the captain's mind of a dangerous or a contagious disease being on board. The men are only described by him to have had headachs and pains in the limbs, symptoms common to most febrile attacks; but yellow skin, black vomit, or any other symptoms characterizing a particular or formidable disease, are never once mentioned. Indeed this could not well have been the case, for if yellow fever had existed on board this ship, it must have arisen from causes existing within herself, as we have the health officers of the place asserting that no such disease was raging at the time she left the Havannah, and in consequence a clean bill of health, of which the following is a copy, was given:

TRANSLATION.

"We, the members of the Board of Health, Dr Antonio Gaston y Navarette, Knight of the Royal Military Order St Hermenegildo, wearing the crosses of distinction of the marine and chiclana, Captain of a frigate in the Royal Navy, and likewise of this port by his Majesty's nomination; Dr Ceriaco de Arango, deputed Chief Magistrate by the most excellent Senate; and Dr D. Lorenzo Hermandes, Physician, Honorary Consultor of the Royal Armies, and First Physician, President of the Royal Medical Senate of Physicians of this always most faithful city of St Christopher of the Havannah, and very loyal Island of Cuba,

"Do hereby certify, That, by the grace of God our Lord, at the sailing from this port for that of Gibraltar, of the Swedish ship Dygden, Captain Gerle, this city and its neighbouring towns are free from all plague or contagious epidemic disease, as likewise the said captain, with the fifteen men of his crew, are in a perfect state of health, according to the muster by her roll, and in compliance with the last orders, we give the present bill of health; Habana, May 9th, 1828. (Signed) CIRIACO DE ARANGO; ANTONIO GASTON; DR LORENZO HERNANDEZ."

But without this assurance of the healthy state of the Havannah at the time the Dygden left that port, it is not probable that the sickness on board was yellow fever, when it is recollected that eight of the men had been about three weeks from port, enjoying good health, and only taken ill when they arrived in a high latitude under a diminished temperature, and exposed to gales of wind and rain. Whoever has been off the Banks of Newfoundland (which this ship must have been at the time mentioned) will agree that it is not a favourable climate for fostering yellow fever. ~~Sir William Pym does not furnish us with any of the data from~~ which he infers the Dygden to be so justly suspected, but proceeds to the next part of his story, and, with the same facility that he carries the disease across the Atlantic, introduces it into our garrison. On the 10th of August, says he, "a boatman, (named Fani,) his son, a daughter, and a boy belonging to the boat, went on board a vessel in the bay, the name of which is unknown, because the father, son, and daughter, are dead. The son, a youth of thirteen, was attacked with the fever on the 11th of August, the next day after being on board, and died on the 17th. His sister sickened on the 17th, and died on the 21st. The boy Caffiero, belonging to the boat, was also taken ill on the 17th, and sent to the Civil Hospital on the 21st, and recovered. The father died on the 7th September. This family was the first who suffered from the disease in Gibraltar."

Now there is something very curious in the whole of this story of the boatman and his family being the means of introducing the fever into the garrison, the truth of which rests solely on the evidence of Caffiero, who was only 12 or 13 years of age, and four or five washerwomen, persons who are seldom capable of distinguishing matters of fact from those of belief, and are never competent witnesses in questions involving both classes of circumstances. It is remarkable that the mother of this family, who is still alive, and more likely to have afforded correct information on the subject, should not have been more fully examined before the Board of Commission. She has, however, subsequently given a very explicit declaration before competent authorities; and which being already published, I can only refer to it here.* This declaration renders it quite clear, that her family had not been on board any ship at the time stated by Sir William Pym; and it being likewise in evidence before the Board of Commission, that Sunday was the day they were said to have gone into the bay, which must be a mistake; for, by the regulations of the garrison, no traffic is or can be carried on with the shipping on that day. Besides, is it likely a man in the situation that this Fani is stated to have been in, would have visited the shipping for pleasure?

But there is another circumstance connected with this story which Sir William Pym omits to mention. It appears in evidence before the board, that on the death of one of the Fani children, a wake was held. I need not describe the nature and object of such an assembly; but I may observe, that, if the children had died of a contagious disease, or if any apprehension of this kind had been entertained by their relatives or neighbours, it is more than probable no such meeting would have taken place. The mother stated, that during the illness of her children people were in the habit of visiting her from all parts of the town to buy cigars; and there is no evidence that the disease was extended by these means.

With regard to the nurse of the Civil Hospital, I would observe, that the nature of her illness at the time created much diversity of opinion; but admitting her indisposition to have been a case of the epidemic, it is certain she again sickened on the 2d of October, and died on the 13th. The woman belonged to the 23d Regiment, under my care, and was in the constant practice of visiting her husband on the different guards; and also a serjeant's wife, who lived in No. 24 district. The servant maid of the Surgeon of the Hospital, I have already stated at page 24, was attacked on the 3d or 4th of August, and relapsed on the 27th of the same month.

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* *Vide* Mr Fraser's paper, Medico-Chirurgical Review.

"*Ques.* 3.—At what period, and, if possible, say on what day, did the first cases appear in Gibraltar? Did they appear at a single point, or did they simultaneously present themselves in different points or districts?

"*Ans.* 3.—The first cases were, as I have said, in a house of 24 district, the situation of which is healthy, very well ventilated, and 200 feet above the level of the sea. On the 11th August the disease spread in the families of the same district, where it continued, and was confined to that part of the town for near three weeks."

OBSERVATION 3d.—I have already shown the grounds on which Sir William Pym's answer rests. If the two Fani children died of yellow fever, it is evident that it excited neither fear nor alarm in the neighbourhood where they lived. That the situation in which they lived is healthy and well ventilated is not correct; the upper floors of the building are tolerably well ventilated; but the poor people alluded to lived on the ground floor, which is not so, and near the door of their apartments was a drain, or rather a junction of drains, which emitted most offensive vapours.

The first well-authenticated cases of the epidemic were a daughter and child of a Mr Martin, who sickened on the 18th of August. The servant maid of this family sickened on the 23d and died on the 28th of August; and (according to the letter of Dr Deas, dated 30th January 1829,) a Miss Maria Testa was also taken ill on the 23d of August; *third*, a Mrs Silcox on the 23d, and died on the 1st of September; *fourth*, a coachman of a Mr Duguid on the 25th, and died on the 31st: *fifth*, a Mr Leachy, four children, and two Jewesses, all in the same house, were seized much about the same time; the exact date not known; *sixthly*, from Mr Fraser's official letter on early cases, a Gullelma Loary appears the next in succession; he was attacked on the 27th August and died on the 2d of September; *lastly*, according to a Dr Mery a Miss Saffity was attacked on the 28th of August, and the first military admission was, I believe, on the 30th of August, from the Sappers and Miners. I have already observed that the fever, for about the first three weeks, followed in its progress the line of drains leading from 24 district, running along the King's Yard Lane towards the convent until checked as it were by the line wall where these drains empty themselves into the sea.

The houses where the first cases occurred are almost in the close vicinity of each other, and from the short time that intervened between the appearance of many of them, it will be no great error in language to describe the disease as appearing at a single point. The locality of No. 24 District I have already hinted at, and quoted Dr Hennen's official correspondence on the subject, showing the density of the population, the crowded and ill ventilated state of the houses, and the accumulation of all kinds of nuisance; and it may not be out of place here farther to observe that this district is situated at the south eastern angle of the town, bounded on the south by Charles the Fifth's wall, which is higher than the adjoining houses; on the east and north by a promontory or projection of the rock, so as almost to form a kind of *cul de sac*, preventing free perflation, except when the wind blows from the west. At the breaking out of the epidemic a commission, as I have already stated, was appointed to examine and report upon the state of this and the adjoining districts; and in their report of the 1st September to the Lieutenant-Governor, in addition to six sheds which they recommended to be razed to the ground, as decidedly inimical to the health of any persons who may inhabit them, "they state that the following tenements, which they enumerate, were found in a state that we conceive unfit for the habitation of man, without undergoing several alterations." And in their second letter of the 2d September, which I have elsewhere given at length, it will be seen that in the adjoining district, No. 25, no fewer than seventeen of these houses are recommended to be destroyed; and they add, as follows: "In the course of our inspection we were struck at every step we took with the density of the population, and we cannot help recommending to your Excellency that the inhabitants be permitted to break out additional doors. and windows in their houses, although not specified in the original plans," and they point out to the scavenging-department several privies into which they ordered lime to be thrown, and urgently recommend Sir George Don to give directions that the openings of all drains, cess pools, and privies, be accurately covered up to prevent the emanation of all vapours from them." So much for the state of No. 24 District, which is stated by Sir W. Pym to be healthy and well ventilated.

" *Ques.* 4.—How did the disease spread from the first point or points where it appeared, and what direction did it follow in the streets or districts?

" *Ans.* 4.—The fever extended to the north and west of the town, having commenced in the south; some points continued healthy until the end of September or beginning of October."

Observation 4th.—It would have been difficult for the disease to spread in any other direction than north-west, the district in which it commenced lying at the extreme south-east angle of the town.

" *Ques.* 5.—Were there any public assemblies when the fever began its progress? If there were any, was it observed that the number of attacks increased in consequence?

"*Ans.* 5.—The churches and places of public resort were shut on the 10th of September, to prevent the propagation of the disease.

OBSERVATION 5th.—How Sir W. Pym reconciles the existence of a contagious disease with the fact here stated I know not. According to him we have the fever first appearing on the 11th of August, and the churches, public places, &c. are not closed for a month afterwards; and although no means are taken during the time to prevent propagation, we have in Answer the 3d, Sir W. Pym's own acknowledgment; and, which is the fact, that the disease was confined to the families of one district for the first three weeks.

"*Ques.* 6.—Has it been noticed if any house or public establishment has isolated itself, taking all possible precautions; and if, consequently, the inhabitants have remained unaffected; or on the contrary, if, notwithstanding this isolation, they were attacked?

"*Ans.* 6.—Two families in the town cut off all communication and were not attacked with the disease, with the exception of a servant maid, who went to visit her sister labouring under the epidemic. This maid was attacked and passed the disease in a room apart, under the most strict observation of some persons, who previously had passed the disease. With this precaution four individuals of the family of Trotabas avoided it. This house is not well ventilated, and is situated in the lowest part of the town. The other family which resided in the town was that of Mr Martinez, composed of eight individuals, one of whom had passed the epidemic, and the others were not attacked on this occasion.

OBSERVATION 6th.—In this answer it is painful to observe the British superintendant of quarantine, on the most equivocal and doubtful testimony, maintaining his favourite doctrine of contagion. The first part carries its own refutation along with it. Two families were in close quarantine,—yet, a maid-servant of one of these families allowed to visit her sister labouring under the epidemic; the thing seems impossible. The fact is, no inference can or ought to be drawn from the facts stated of these two families. By the evidence given before the board of commission on the subject, and which I here subjoin,* it is clear nothing like complete seclusion

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* "Evidence of Trottabas, &c. before the Board of Commission.

"*Ques.*—Did you reside in the town of Gibraltar from August to January last? What did your family consist of? Had any of them passed the fever before, and was any of them sick during the late epidemic? Did you take any precautionary measures of seclusion, and what were they?

"*Ans.*—I did reside in this town during the time mentioned. My family consisted of thirteen persons including servants, five of these had passed the fever during various epidemics in this garrison, and two had passed it in Spain. One servant maid existed; and the immunity which they are stated to have enjoyed is more satisfactorily accounted for on other grounds; the situation of their houses, perhaps, one of which is situated at a corner where four streets meet, and where there was free ventilation. There were also several families, neighbours of Martinez, who never put themselves in quarantine, and who never caught the disease, showing that seclusion was not the cause of the immunity enjoyed by this family. Trottabas was a baker, and carried

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was ill of the epidemic fever in the latter end of October last. She had not passed the fever; she passed the disease in my house. I placed this woman in a distinct and separate room, which I had prepared to meet such an accident. This room was well ventilated and separate from the rest of the family, and was entered only by my mother and myself while this woman was ill in it. We had passed the fever before. Fearing least we might communicate the disease to the rest of the family, my mother and I always took the precaution either of washing ourselves with vinegar, or changing our clothes upon leaving the sick room, before we mixed with the family again. These precautions we observed to a greater or less extent according to the communication we might have had with the sick person. The illness of this woman lasted seven days, but she was kept without communication with that part of the family that had not passed the fever for twenty-five or thirty days, and even at the end of that period she was not allowed to mix with that part of the family until she had been washed over with eau de Colonge twice.

"The clothes she had used during her illness were hung out in the open air until she was sufficiently recovered to wash the same herself; the room was afterwards perfectly white-washed and fumigated. No other individual of my family was ill during the last epidemic.

"My house is situate in the lower part of the Main Street, near the Four Corners. It is one of the oldest houses in Gibraltar, and is not free from bad smells. There are two privies and two sinks in this house, which emit a very bad smell, in consequence of their level being below that of the street. My shop being open, people came freely into it, but they did not mix with the family, particularly with that part of it who had not passed the fever. I took particular care that my four children, from the age of seven to one year old, should not leave the house except under charge of a person who had passed the fever, and who would prevent them from having any communication, direct or indirect, with sick people. My servant maid was attended in her illness by Dr Mery. My children went out sometimes during the day, but chiefly from sunset to dark, this being my leisure hours. I have reason to believe my servant maid had visited her sister, who was ill of the disease previously to her falling ill herself, and contrary to my orders."

"Evidence of Mr Laurioux de la Sotilla, clerk in the house of Ignacio Martinez.

"*Ques.*—Did you reside in the town of Gibraltar from August to January last? How many did the family you lived in consist of? Had any of them passed the fever before? Were any of them sick during the epidemic, and were there any measures of seclusion adopted in that family?

"*Ans.*—I did reside in the town during the time mentioned. The family in which I lived consisted of eight persons, seven from the age of thirty-four to eleven, and one, the maid-servant, was fifty-four. She was the only person who had passed the epidemic. No individual of this family was ill during the epidemic. The whole of the family remained in town during its continuance.

"Mr Martinez, myself, and the servant boy, were the only persons who quitted the house during the epidemic. Mrs Martinez, the children, and a lady who lived with her, were constantly shut up at home. Mrs Martinez's house is situated in the Main Street, No. 24, opposite Tukey's Lane. No visitors who had not passed the fever were received, and even those who had passed it, who might have visited sick persons, were not received. Mrs Martinez and her children went out for the first time on the 14th of February. Mr Martinez and myself were never out after half-past seven or eight o'clock. We avoided as much as possible those who had a sickly appearance. There are two drains in our house; they don't give a bad smell. The house is a new one, and was finished in September last.

on his business, keeping open shop during the whole period of the epidemic—another proof that his seclusion could not have been very perfect. But, willing to give Sir W. Pym the full benefit of these two families as strengthening his doctrine of contagion; I could, were it necessary, quote fifty others having an opposite tendency. The family of a Mr Farquhar, for example, who lived below the Naval Hospital, put themselves in strict quarantine at the commencement of the epidemic, one of whom caught the disease and died. The examples of people enjoying immunity from situation alone, though in constant communication with the sick, might be multiplied without number. In the same house even, families occupying the upper stories were known to escape while those on the ground floor suffered severely.

In Bossana's house at Rosia, for example, many individuals in the upper story escaped the disease, while every person in Belasco's family, living directly under them, and who had not passed the fever, were attacked. In another house in the south, sixteen individuals died on the ground floor, and not one was taken ill in the upper.

"*Ques.* 7.—Has the same been observed with respect to any district or wards? In that case indicate the peculiarities of the locality.

"*Ans.* 7.—Three families in the south district of the garrison also exempted themselves by the same precaution, namely, those of Mr Howell, Major Drewe, and Mr Pritchard, although the disease was prevailing in all the contiguous houses."

OBSERVATION 7th.—How far seclusion was effected in any of the three examples quoted by Sir W. Pym, the reader may judge when he has perused the evidence as given before the Board of Commission. Of Major Drewe I am not exactly certain. His evidence, for what reason I know not, does not appear on the face of the proceedings; but the impression on my mind is, that I have seen him several times visit the Hospital, and performing the other duties of a field-officer during the fever. Mr Pritchard's evidence speaks for itself, and I shall leave it as it is; * and Judge Howell

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* Evidence of Edward Pritchard, Esq. Registrar of the Civil Court.

"*Ques.*—Did you reside in this territory in the south, from August to January last? What did your family consist of? Had any of them passed the fever? Were any of them ill during the last epidemic? And did you adopt any, and what, measures, and when?

Ans.—I did reside in the south during the time mentioned. My family consisted of myself, my sister, and three servants. One of them only, the man-servant, had passed the fever before. None of my family was ill during the last epidemic. We commenced to seclude ourselves at the latter end of September. The seclusion was perfect, with the exception of the man-servant, who had passed the fever before, who did our marketting in town for about two months after our seclusion; after which this man was permitted to go out on Sundays only, to visit his family.

"*Ques.*—Did you receive any visits during the period of your seclusion?

"*Ans.*—As far as I can recollect, we were visited by Ensign Lewis only, 94th Regiment, who was quartered on Wind-Mill Hill. Mr Amiel also called to see my authenticated before the Board, of which he was a member, the evidence he had previously given to the Anglo-French Commission, relative to the seclusion of his family, which was to this effect; namely, that he had not secluded himself; that he was in the constant habit of receiving people on business into his house; paymasters of regiments for example, to swear to their pay lists; and that Mr Amiel, surgeon of the 12th Regiment, had been in the habit of visiting his family often, directly from the wards of his hospital.

" *Ques.* 8.—During the prevalence of the said fever in Gibraltar has there been noticed any particular phenomenon among domestic animals, quadrupeds, or birds?

" *Ans.* 8.—It is said that several dogs have died. A monkey which belonged to Mr Griffiths died after three days illness, and the skin became yellowish."

OBSERVATION 8th.—In addition to the dogs and monkey, a goatherd lost a great part of his flock, and almost the whole ceased to give milk; and a Mr Bonfante, in the course of ten days in the month of September, lost one mackaw, two parroquettes, one parrot, three canary birds, one calandra, one raccoon cat, one small monkey, and one ring-tailed monkey. Three pointer dogs were also taken ill with shivering and great debility, and they were constantly vomiting at first a kind of bilious matter, afterwards a very black matter; the eyes and ears were quite yellow. Mr B. adds, that the dogs survived in consequence of quantities of oil having been given to them. Mr Duguid also lost eight game dogs within a few days of each other. All were yellow, and vomited; and, in the course of the epidemic, upwards of twenty parrots were sent to Mr Fraser of the Civil Hospital, from inhabitants to be dissected.

" *Ques.* 9th.—Has the fever attacked any individual who had suffered from it formerly in Gibraltar, or in any other place?

" *Ans.* 9th.—I do not know that any person who has suffered from this disease in the West Indies, America, or Spain, has again contracted it in Gibraltar. There were upwards of 6000 persons who had laboured under it previously, and these have not been attacked a second time."

OBSERVATION 9th.—If Sir W. Pym had shown the candour of a

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sister, who was slightly indisposed, and also to see the servant-maid. Miss Dantoine, a neighbour, also called. She had passed the fever in 1804.

" *Ques.*—Were there any inhabitants of the houses immediately around your house attacked during the epidemic?

" *Ans.*—Yes; I understand there was sickness in every house around me. A great many deaths took place, particularly in the hospital road. Miss Dantoine did not visit us after the servant-maid died, as far as I recollect.

" Thomas Jones Howell, Esq. Judge, Member of this Board, requests that the information given by him to the Anglo-French Commission, relative to the seclusion of his family, may be read, when he will authenticate it."—See Proceedings Board of Commission.

man anxious to elucidate the truth, he would have mentioned the case of Boyd of the 23d R. W. F. who died of the late epidemic after having had yellow fever in the West Indies. Many other well-authenticated cases of second attack could be brought forward. Sir W. Pym invariably showed an unreasonable degree of incredulity when those cases were mentioned to him, though he was very anxious to receive, however equivocal, the testimony of those who favoured his own view of the question. The fact is, the 6000 persons here stated as having had the fever formerly, and who had not been attacked a second time, rests on questionable evidence. After what I have stated of the Dygden, the Fani family, Trottabas, and other persons who put themselves in quarantine, &c. &c. it will be perceived that Sir W. Pym is not scrupulous as to evidence on one side of the subject. But I am not inclined to cavil with him on this point. The non-liability doctrine is proper enough to be promulgated amongst the people, for it excites courage. I only contend the rule is not absolute; but, as Dr Hennen justly observes, "only a general result." How Sir W. Pym claims this as his discovery, is to me inexplicable. The same fact was publicly proclaimed in the streets of Malaga one year before he ever thought of such a thing. Arejula, in 1803, had the following placard posted in the different streets of that city; and Humboldt mentions in his Political Essay, that at Vera Cruz, "in the midst of the epidemical black vomiting, the natives and strangers seasoned for several years to the climate enjoy the most perfect health;" and I may say almost every writer on yellow fever states the same fact. The degree of immunity, however, conferred by first attacks, is yet to be ascertained, as well as the difference between relapses and second attacks, the former of which were of frequent occurrence after our late epidemic.

"Estaran Todos en la firme persuasion de que el que haya sufrido ya la enfermidad epidemica en esta ocasion, en el ana pasado de 1800, en qualquiera otro, o permaneceido largo tiempo en la America Saptentrional, *esta exeuto de este mal;* y debe suaderle, para que lo entienda bien, como al que ha tenido viruelas, que no rezela m piensa en que se le pequen, aun quando entre adoude les hay: por tanto, debe este a fortunada clase de sugetos uo huir del riesgo, pues uo tierie el menor, y prestarse con gusto y satisfaccion a la asistencia de sus concindadanas y parientes."—See Arejula Fiebre Amarilla, page 229.

"*Ques.* 10.—In preceding years, at the same season that the prevailing fever began to break out, have fevers similar to this been also observed, if so, point out all their analogies or points of resemblance?

"*Ans.* 10.—In the years preceding 1828, during the months of August, September, and October, nineteen soldiers only have died of fever. When remittent fevers appear in Gibraltar, which is rare, they are traced to marshy exhalations, formed along the small streams in the vicinity of San Rogue. Intermittents are not known in Gibraltar. A solitary instance has been observed during the late epidemic in an individual who had come from Barbary."

Observation 10th.—Sir W. Pym does not mention the nature of the fever of which these nineteen soldiers died. At one period of medical rule in this garrison every variety of fever was ordered to be returned under one head. Such, indeed, was the thraldom of the military medical press (if I may so use the expression) in Gibraltar, from the termination of the epidemic fever of 1814, until the arrival of the late Dr Hennen in 1826, that it was considered a most wicked heresy for a surgeon of a corps to return fevers under any other head than simple continued fever. The consequence is, that although febrile diseases are the most frequent of the numerous diseases treated both in the Military and Civil Hospitals, no correct table of fevers can be now framed from the returns of these establishments; no distinction whatever being drawn between the different species of remittent and continued. The authors of such a measure can best answer for themselves. To me it appears to have been an attempt, and one too successfully made, to obstruct the advance of truth, short of nothing but the direct application of the bayonet's point; and it is due to the memory of Dr Hennen to state, that he had not assumed the charge of the department two weeks before he abrogated this mischievous state of things. This measure soon showed, that neither the intermittent, remittent, or continued yellow fever, were so uncommon forms of disease in the garrison as Sir William Pym, and others of his school had for a series of years endeavoured to represent. Instead of remittents being rare, and intermittents never occurring in Gibraltar, Dr Hennen says, at page 119 of his Medical Topography, " I have no hesitation in asserting, that remittent fevers are of frequent occurrence (speaking of Gibraltar;) that the bilious autumnal remittent is annually met with, and that cases of genuine yellow fever, (as described by writers on the diseases of the West Indies) accompanied with true black vomit, occur both in the Civil Hospital and in private habitations." And a little farther on in the same paragraph he adds, " It has been asserted that these fevers are solely the produce of the Neutral Ground and the Lighters in the bay, but annual proofs subversive of this hypothesis are not only familiar, but on record." I can with confidence add my own testimony to the truth of these observations. I have already this year (1830) seen three well-marked cases of sporadic yellow fever in this garrison, two of them with black vomit.

" *Ques.* 11.—During the prevalence of the fever, which has committed such ravages in that town, have any other acute diseases peculiar to the season and different from it been observed? In that case mention what they were, and if their usual character has undergone any modification.

"*Ans.* 11.—Gibraltar is generally very healthy. No other particular disease was experienced during the epidemic. There have been some fevers among the soldiers, arising from exposure to the sun, or other irregularities. In general they were sent to Hospital, as if they had been attacked with the epidemic; and, consequently, some of them, convalescents from that complaint, were actually attacked by the epidemic, and this was considered a relapse of the latter disease."

Observation 11th.—Why Sir William Pym believes that the cases admitted into Hospital were simple fever arising from the common causes, exposure and irregularity, and not that they were cases of the epidemic, I candidly confess I know not; for I can assert I never saw any of these cases of simple fever, and I was present from the beginning to the end of the epidemic. Never were soldiers less likely to have fever from exposure and irregularity than during the epidemic season, for all parades and duties, except mounting guards, were suspended. The men were strictly confined to the camp, so that they could not indulge in their wanton habits of drunkenness and dissipation; and from the almost total subsidence of other diseases during this period, I am borne out in the opinion, that all the fevers then admitted were of the same nature, and that when relapses took place they were fairly to be considered as relapses of the epidemic fever, and not this disease supervening after an attack of simple fever. I may here mention a fact illustrating the general epidemic influence, and which was the total disappearance of ophthalmia in the 23d Regiment, in which it had for some time extensively prevailed, during the prevalence of the fever, and its reappearance when the febrific miasmata ceased to produce their effects.

"*Ques.* 12.—Has the prevailing fever attacked any individuals labouring under chronic affections, such as venereal, scrofula, rheumatism, phthisis, &c. and if there have been any patients of this kind, has it been remarked that their former complaints underwent any modification?

"*Ans.* 12.—Few cases of venereal or scrofula existed in Gibraltar, but several of those who had pulmonary complaints were attacked by the epidemic, and in some cases they seemed to be cured of these affections."

Observation 12th.—I am unable from personal experience to confirm or refute what Sir W. Pym here states, as the few men in the 23d Regiment who laboured under other diseases at the time they were attacked with the epidemic fell victims to it. It was, however, generally observed, that this fever was less fatal amongst the weakly and delicate than the robust and strong.

"*Ques.* 13.—Has any circumstance worthy of remark been observed with respect to pregnant women, or those in child-bed, or in new born infants, who have been attacked with the prevailing fever?

"*Ans.* 13.—This disease was very fatal to pregnant women, as several miscarried."

Observation 13th.—I believe a great majority of the pregnant women who were attacked with the epidemic fever miscarried, and many died. One woman died in the Civil Hospital, and was examined after death; and although in her own person the usual appearances of the fever were present, the child and contents of the uterus were perfectly healthy. The fluid found effused into the abdomen of the mother was deeply tinged yellow, but the *liquor amnii* was clear and transparent, and the skin of the child of a natural colour. Fever of any kind frequently occasions miscarriage.

"*Ques.* 14.—Were the attacks most frequent in the day or during the night?

"*Ans.* 14.—The attacks were more numerous during the night, according to the morning reports."

Observation 14th.—I should say, as far as my own observation goes, that four-fifths were attacked at night, and, as I have already observed, some particular guards sent more men in proportion to hospital than others.

"*Ques.* 15.—When the atmosphere presented any variation in its qualities, was there any variation in the number of the attacks, or did those already attacked experience any alteration?

"*Ans.* 15.—When the winds were light the attacks were more numerous, but the symptoms did not vary."

Observation 15th.—What the particular atmospherical conditions were which influenced the progress of the epidemic have not been satisfactorily ascertained. Temperature had evidently a more marked share in checking and ultimately destroying the disease than winds or rains, as we had frequently strong breezes from the southward and westward without producing any perceptible effect. We had heavy rains also without the disease being at all checked; but the moment the temperature became reduced, in consequence of a northwest wind, the fever subsided, and in one night I may say the febrific poison ceased to exist.

"*Ques.* 16.—Has the fever been more frequent or more fatal among the military than among the civil population of the garrison, and has it made greater ravages among the poor, and where cleanliness was neglected, than among the rich and well lodged? Point out the causes which may have produced these differences.

"*Ans.* 16.—The mortality among the rich and poor were equal, as also the proportion of deaths among the military and civilians, excepting two regiments, viz. the Royal Artillery, and 43d Regiment, which suffered more in proportion than any other corps, as nearly one-half of those attacked died. I believe the general average of deaths has been one in 4½."

Observation 16th.—This is in unison with other general assertions in these answers, and where the superintendant procures his data would be, I apprehend, as difficult for him to say, as it is impossible for me to conceive. There were no documents on the subject to enable me to demonstrate the contrary; but of this I am certain, that it was the general opinion, not only of the medical officers of the garrison, but indeed of all the inhabitants, that the mortality was much greater among the poor. The admissions amongst the military, including officers, women, and children, were 2002, and the deaths 512, or about 25.5 per cent.; estimated number of civilians admitted 4701; deaths 1281, or about 27.2 per cent., making a slight difference in favour of the military. I think it necessary to notice this fact, as it contradicts the assertion of another individual, Dr David Barry, of Welbeck Street, Cavendish Square, London, who does not appear more scrupulous in weighing evidence than his patron Sir William Pym. This gentleman has asserted that the practice of the Spanish civilians was more successful than that of the British army surgeons. Upon what foundation is this opinion stated?

"*Ques.* 17.—Upon the whole of the patients can the disease be viewed under distinct gradations or forms? Point out the principal symptoms in each of these gradations or forms, as also their respective mortality.

"*Ans.* 17.—I was so much engaged in the duties of medical superintendant, that I could not fix my attention on particular cases. The disease, however, appears to have presented itself under three varieties. 1st, The patient was slightly attacked, the paroxysm lasted from fifteen to twenty hours, and the convalescence was rapid. In the 2d, the paroxysm was prolonged to fifty or seventy hours, and terminated quickly, the symptoms having risen to a greater height than in the former; the patient was apparently convalescent when the second series of symptoms appeared; namely, a sensation of flatulence in the stomach, hiccough, sickness, and vomiting of a dark gray fluid; afterwards, black, like coffee-grounds; incontinence of urine. At the latter end of the third day a yellow tinge appeared in the neck and eyes, and the patient generally died on the fifth or sixth day. Very few who had these symptoms survived the disease. In the 3d variety, the disease was more violent from the outset, being attended with delirium, great irritability of stomach, black vomit on the second day, and death on the third.

Observation 17th.—I am inclined to agree with Sir William Pym in describing this fever under three forms or varieties, and I would say, that each variety was characterized by its own peculiar symptoms. The first or mild form did not differ from fever arising from common causes, except that the lumbar pains and frontal headach were perhaps better marked; the tongue was often clean; the circulation little disturbed, and the heat of skin moderate; and from twenty-four to thirty-six hours the patient was generally restored to convalescence. Relapses were more frequent after this form of the disease, and often proved fatal. In the second or more severe form, the symptoms were more concentrated; the lumbar pains more acute; the frontal headach more intense; the pulse strong and quick; eyes suffused, and often of a dingy colour; thirst; a dry and red tongue, sometimes moist, and coated with a white viscid matter, sometimes in streaks, but more frequently covering its whole surface; vomiting not unfrequent; and, as the fever advanced, the skin became yellow as well as the conjunctiva, and hemorrhages from the nose, mouth, or anus occasionally supervened; delirium also; sometimes coma, with a sense of burning in the throat, and tenderness of the epigastrium; the exacerbations were often well marked, but the duration of the fever was quite uncertain, depending apparently on peculiarity of constitution and other causes. I have known upwards of twenty days elapse before convalescence was established, and in some of these cases delirium, perhaps more properly stupor, was present the greater part of the time. I seldom observed this form prove fatal; on the contrary, when the bowels were freely and easily acted on in the beginning, and where the vomiting was not constant, recovery was almost certain to follow. Relapses were not frequent after this form. The last, and what I would call the fatal variety, presented itself in a more aggravated form from the very beginning than any of the other two. There was great prostration of strength, with vomiting, anxiety, quick and often irregular pulse; tongue dry and furred; a peculiar dull and suffused appearance of the eyes, and countenance, altogether not unlike that of a drunken person; restleasness and often severe pains in the calves of the legs and whole lower extremities; bowels difficult to move; thirst urgent, and in a short time black vomit; suppression of urine; hiccough; yellow skin; hemorrhages, *petechiæ*, sometimes *vibices*; delirium; coma; a deadly or icy coldness of the extremities; pulse imperceptible, and soon after convulsions and death. This form was also uncertain in its duration; but I have never known it extend beyond the fifth day, and in one or two instances it proved fatal in thirty-six hours.

" *Ques.* 18.—What method of cure has been most generally employed, and with the least doubtful result? Give some particular cases, principally of those patients which, in the progess of their diseases, have presented phenomena worthy of remark.

" *Ans.* 18.—Many practitioners used mercury; but in my view, many of those who recovered under its use would not have failed to do so if they had not taken any. Purgatives and laxatives employed at the commencement were more successful. Oily remedies, castor oil, or olive oil, it is said, have been used with much advantage by some families who did not call in any medical man. It is asserted that many individuals who concealed their diseases for fear of being removed to the lazaretto were all cured by using oil."

OBSERVATION 18th.—The whole of this answer is an assertion without proof. Sir W. Pym has told us that his superintending duties prevented him from attending to particular cases; yet he scruples not to condemn mercury, as useless at least, if not injurious. My experience leads me to a different conclusion. I have seen it in the severe forms of the fever decidedly beneficial. The majority of the cases, when the system was fairly under the influence of this medicine, did well; and I think without its use several of them would have had a different termination. In the mild forms mercury is not called for. Beyond keeping the bowels moderately free, nothing is required; and I have known several patients get well without the use of any medicine. In the more severe forms, however, allaying morbid action, and relieving urgent symptoms, were imperiously indicated; and with this view, especially at the commencement of the epidemic, bleeding was freely practised in some corps, but, as far as I know, without any very good effect, a small quantity abstracted often producing alarming debility. The application of leeches, however, to the epigastrium, when there was pain or tenderness, I have seen useful. But the most successful practice appeared to be that which kept up for the first two days a free alvine discharge. Warm bathing; blisters to the epigastrium; large enemata, and frequently repeated frictions to the extremities, &c. &c. were all occasionally had recourse to, and sometimes with advantage.

"*Ques.* 19.—Were disinfecting means used, such as fumigation, washing, ventilation, &c. and among these the solutions of the chlorurets of sodium and lime, either in public establishments or in private houses? Point out what these means have been, and likewise their effects or results.

"*Ans.* 19.—At the commencement of the disease, fumigations with nitre, manganese, and acids were used; afterwards the chlorurets of the oxides of sodium and lime. The last two were used in great quantity to purify the houses after the termination of the disease; but as the fever was so general, it is impossible to state the effects produced by them. Of those who attended the patients in the hospital, few escaped, especially those who were employed for some time on that duty."

OBSERVATION 19th.—I have never known any good effects result either from fumigations or the use of the chlorurets. The latter were used in the dissecting-rooms with the view simply of destroying bad smells; but of their disinfecting powers I have no knowledge; and the employment of these articles in expurgating the town must have been useless, as this process, to my knowledge, was very imperfectly performed.

"*Ques.* 20.—Have autopsies been made? Point out with the greatest exactness every thing noticed in the subjects, as well internally as externally, joining to the inspection the particular history of each case.

> "*Ans.* 20.—A great number of bodies were inspected. Externally, yellowness of skin and spots in certain parts of the body were only remarked. Internally, the stomach and the liver were the organs injured. The liver presented yellow stripes like variegated marble. The internal coat of the stomach presented different aspects; sometimes it appeared as if separated, and with portions abraded; at other times were observed gangrenous spots, which extended from the stomach to the duodenum."

OBSERVATION 20th.—This is such a meagre account of the necroscopic appearances that I shall give the following detail drawn up by my excellent assistant Dr Browne, from a great variety of autopsies carefully made. *Externally.* Yellowness of skin, ears of a leaden colour, depending parts livid; dark spots on the anterior part of the scrotum, with occasional excoriations. Yellowness generally extended to the osseous, adipose, and other structures, with the exception of the muscular. *Head.* There was usually an infiltration of a yellowish serosity to a greater or less extent under the arachnoid, with congestion in the veins and sinuses, though in some cases this was not very remarkable. *Thorax.* The lungs presented, almost always, numerous dark spots externally; or were nearly uniformly of a dark colour, and the infiltration of blood into their tissue, forming what is called splenization, was very frequent. The bronchial mucous membrane was often reddish and coated with a copious sanguinolent frothy fluid; in other instances it was pale or yellowish, as also that of the trachea. *Abdomen.* The contents of this cavity were almost always more or less of a yellow tinge. The œsophagus was usually deprived of epidermis near its termination, and exhibited spots of a dark or brownish colour. The stomach, nearly in every case, contained black vomit, and was often distended with that fluid or inodorous gases. Dark patches in its mucous coat, of various extent, were found around the cardia especially, and more or less of punctated redness in the large extremity and large curvature, from injection of villosities. In the rest of its extent it was of a rose shade, and in a few cases it was of a pale or rose colour throughout; it was always coated with a copious viscid mucus, and often presented numerous rugæ, with a more or less marked *etat mammelonné* in their intervals. Its tenacity was but little diminished in many cases, and only round the cardia or in the large extremity. It occasionally exhibited slight circular depressions, as if a bit of the mucous coat had been pinched out, very rarely ulceration or thinning, and it never was gangrenous, or separated from the subjacent tissues. The small intestines, in the majority of cases, presented more or fewer vascular arborizations externally, and a slight tinge of yellow. Internally, they usually contained a dark fluid, which increased in consistence as we descended in the canal, but in a few instances the contents of this part of the tube resembled pease-soup, or a yellow glairy matter, more or less consistent. Their villous coat was generally covered with a copious viscid mucus, and sprinkled with patches of punctated redness, especially in the duodenum; and the free margins of the *valvulæ conniventes* were frequently reddish. It was often more or less softened, but never presented alteration of the glands of Peyer, or ulcerations towards their termination. The large intestines contained generally a pultaceous black matter, and their mucous coat was, ordinarily, of a natural appearance and consistence. The liver was almost in every case of a yellowish or pale colour, resembling boiled liver, and the red granulations were not so distinct as usual; its tissue did not contain much blood, nor did it exhibit any traces of coagulable lymph or suppuration. The gall-bladder, generally, contained a greater or less quantity of a dark glairy fluid, resembling molasses. In some few instances, however, the contents of the viscus were clear and transparent, and in one or two cases the ducts were obstructed. The spleen was commonly of the natural appearance. The kidneys were more or less yellow. The bladder was generally contracted and empty, and coated internally with a yellowish mucus. At times it presented more or fewer vascular arborizations towards its cervix.

The muscles were usually of the natural appearance; but in a few rare cases, there was an infiltration of a gelatinous or sanguinolent fluid into their substance, which was remarkably softened. At times the psoæ and internal iliacs were of a dark hue, and softened.

" *Ques.* 21.—A short topographical description of the inhabited part of what is called Europa Point, and the Encampment at Land Port is requested; that is to say, What distance and in what direction they lie from the town; if the ground is flat, or uneven, or chalky, or sandy; if there are pools or hills, &c. in their vicinity, and their respective situations?

> " *Ans.* 21.—Europa Point and Wind-Mill Hill are a long mile from the town wall, and form the most southern part of Gibraltar. The bottom is of the same limestone as the rock, with very little sand or vegetable earth, and from one to 200 feet above the level of the sea. In this locality the wind from the east gives a free circulation, which beats against the more perpendicular and elevated side of the rock, nearly two miles in length.
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> " The Encampment at Land Port begins about half a mile from the gates of the town, on the Neutral Ground, which, from its situation between the rock of Gibraltar and the high land of Spain, enjoys constantly a free ventilation. The soil of the

> Neutral Ground consists chiefly of sand and shells, with the exception of some parts where gardens have been formed artificially, by the accumulation of manure collected in the streets of Gibraltar. In this locality there are no pools or marshy spots. The only point nearest Gibraltar which may be considered unhealthy is Campo Mento, half way to San Roque. Intermittents have never been generated in Gibraltar; and a few cases, though very rare, have been observed on the Neutral Ground, among families who live there or near the gardens."

OBSERVATION 21st.—The description here given of the Neutral Ground by Sir W. Pym is somewhat different from Dr Hennen's, and certainly at variance with facts. Instead of there being no pools or marshes, as stated by the former, Dr Hennen says, at page 22 of his Medical Topography, and which is the truth, "in winter the rain water forms numerous and extensive pools, which continue during the spring months. These pools are only dried completely up by the summer heats; but there is one near the high road beyond the gardens which is never dry."—"Besides these adventitious depositions of moisture," says he, "there are numerous internal sources of permanent supply at the Neutral Ground. I have been led particularly to examine the spot, in consequence of the assertion that no source whatever of marsh miasmata existed at Gibraltar. If, by this assertion, it is meant that NO MORASSES at present exist, I perfectly concur with it, but I can go no farther; because, under the present head, I have indicated numerous sources of aqueous exhalations, and I am about to point out others of a still more extensive nature."—"Indeed," continues he, "the most superficial observer could scarcely ride over the Neutral Ground without perceiving many external evidences of under-ground moisture." I might here continue this quotation illustrative of the same fact, but enough has been given to show the inaccuracy of Sir W. Pym's statement. The last clause of this answer, respecting the non-appearance of intermittents in Gibraltar, I have in the 10th observation satisfactorily refuted.

"*Ques.* 22.—What proportion of the population withdrew to these two places when the fever appeared in the town?

"*Ques.* 23.—Did they live in houses or huts dispersed, or collected and forming streets?

"*Ques.* 24.—Did all these people remain without communication with the town, or did they keep up communication by means of some individuals going to and fro?

"*Ans.* 22, 23, 24.—About 6000 of the population of Gibraltar withdrew to the Neutral Ground. Those who passed the epidemic in preceding years kept up free communication with the town. They lived in detached houses and in tents. The military for a long time kept up communication with the town doing their duties and guards, &c. until a sufficient number had recovered from the fever capable of performing that service, then the posts were occupied by soldiers only who had passed the epidemic."

OBSERVATION 22d, 23d, 24th.—Free communication was kept up until the time of Dr Hennen's death in November, between the town, the Neutral Ground, Europa Flats, and the Bay, but no well authenticated case is known to have originated in either of these two latter places. Neither could the disease be propagated there by contact or other means. I have known matter of black vomit deposited in the bed of a person in health without producing any bad effect; and people, just arrived from England, and, of course, peculiarly susceptible of the disease, act as nurse to a patient on the Neutral Ground in the last stage of this fever with black vomit, &c. with impunity; and I have already enumerated other facts corroborative of the truth of this assertion; namely, that our late epidemic could not be propagated in any of these localities; and I have also stated, that all those who strictly confined themselves to the above localities remained healthy.

" *Ques.* 25.—In these two encampments or villages were there any patients with the same fever as in the town?

" *Ans.* 25.—The same disease was introduced among different families in the Neutral Ground encampment, but this was from their communication with sick or contaminated individuals."

OBSERVATION 25th.—It certainly was introduced into these encampments; but it is well known that the individuals alluded to contracted the disease out of camp, and did not communicate it to their comrades.

" *Ques.* 26.—Have those persons who, at the beginning, left the fortress, and withdrew to the points referred to, maintained themselves exempt by avoiding all communication with the town?

" *Ans.* 26.—They remained unaffected if they had no communication with the sick or contaminated persons."

OBSERVATION 26th.—This reply does not require to be particularly noticed.

" *Ques.* 27.—Of those who, though encamped at the beginning, continued their communication by means of persons or effects, were any attacked with the disease in question?

" *Ans.* 27.—There were some cases of certain persons having been attacked after having had contact with patients on Wind-Mill Hill and Europa Point. These cases were more frequent on the Neutral Ground."

Observation 27th.—This answer is incorrect, and requires no comment after what I have already stated.

"*Ques.* 28.—If some really were attacked, did they communicate the fever to those encamped close by them?

"*Ans.* 28.—The sick communicated the disease to others on the Neutral Ground in *one* or *two* instances in houses full of people. I may mention the family Boufanté; but what is strange is, that in Catalan Bay, on the east side of the rock, the fever did not reach it until the 25th or 27th of September. The first person attacked was a water-carrier, (named Andrea Andre,) who had communicated with the town. The disease spread from this man to his family and others who resided in Catalan Bay, who had not entered Gibraltar for some weeks previous, as was the case with about 100 families who live in that spot: Nevertheless, the disease only spread to ten families, and to about forty individuals."

Observation 28th.—Sir William Pym in this answer contradicts himself. In the answers, 25–27, he tells us the cases were frequent on the Neutral Ground where persons were attacked who had communication with infected individuals, but here the examples are reduced to *one* or *two*, and only one family mentioned by name. A more unfortunate example perhaps Sir William Pym could not have chosen. According to the evidence given before that Board, a young lady faints, and is instantly seized with an alarming fever at the very sight of what she calls black vomit on the skirts of the garments of the parish priest. A loving swain follows her example; and the padre, the author of the mischief, gives a flat contradiction to both their stories. He states, although not examined before the Board, (for reasons that are obvious,) that he had no black vomit on his clothes; that he had not been visiting any person labouring under that form of the disease; and that, the only person he had seen was a Mr Testa of the Health Guard, who was suspected by the importationists of introducing the disease into the garrison, and that he was in a tent by himself. But our astonishment is not allowed to cease here at the magical powers of contagion. Sir W. Pym gives a brief outline of its introduction into Catalan Bay, and how it spread from the unfortunate waterman, Andrea Andre, in this little village; but, as I have already referred to this story (page 22,) in speaking of the origin and progress of the disease, and shown how far contagion operated in the business, I shall take no farther notice of it.

"*Ques.* 29.—When the fever broke out in Gibraltar, were there any persons or families who took refuge on board vessels anchored in the bay?

"*Ques.* 30.—Has the disease been observed among those refugees, or among the crews of the vessels where they went to live?

"*Ques.* 31.—Were there any vessels which put themselves in strict quarantine? In his case has it been observed that they had patients with the same fever that prevailed in town?

"*Ans.* 29, 30, 31.—Several families embarked in vessels lying in the bay, but I have not known of any one having been attacked. The captain of the port established excellent precautionary measures for the vessels. In 1804, such measures were not taken, and there was several cases in the bay."

OBSERVATION 29th, 30th, 31st.—Several hundreds embarked on board vessels in the bay, and as many I believe had free communication between the shore and the shipping during the epidemic. No precautionary measures whatever were adopted by the captain of the port. I myself had almost daily communication with a ship in the Bay, for the purpose of visiting my commanding officer's wife, who removed thither at the breaking out of the epidemic, yet the disease never spread among the shipping. Probably Sir William Pym never expected his replies to the Spanish faculty would have been made public. Certain it is they were printed in Cadiz five months before the circumstance became known at the Rock, and it was always difficult to procure a copy. I had very great difficulty in procuring the one I did; and I am told it is now impossible to purchase one. All the copies, it is said, have been purchased by the superintendant.

"*Ques.* 32.—And of those who continued in free communication with the garrison, were there any of them sick, or did they continue free from the disease?

"*Ans.* 32.—The precautions on board the vessels were most rigid; and I do not know that any one has been attacked with the fever on board."

OBSERVATION 32d.—This answer demands no particular observation. If any individuals sickened in consequence of communication with the shore, they never affected any one else on board the same ship with them.

"*Ques.* 33.—It will be expressed if the individuals who dwelt on board the vessels where these were, no sick had passed the fever at former periods?

"*Ans.* 33.—Of the individuals on board, some had passed the epidemic; but 400 people who had not passed it went there likewise.

"It appears that a considerable number of individuals passed the epidemic in their mother's womb. When the latter had it in preceding years, they enjoy the same immunity against a second attack of yellow fever; and I am of opinion, that the children of those fathers who have passed this disease will suffer less from it than those whose fathers have never had it."

Observation 33d.—It is not likely that many in the Bay belonging to vessels had passed the fever; and of the 400 who left the garrison, according to Sir William Pym, I am not aware that one suffered from the disease who constantly remained on shipboard. The last and concluding observation in this answer is so purely hypothetical, that it scarcely deserves notice. The autopsies I have alluded to at the Civil Hospital would seem to negative the idea of children passing the epidemic in their mother's wombs, when the latter had it in preceding years; and that of the children of those fathers who have passed the disease suffering less than others, is, I suspect, an opinion resting on the same visionary data as many others advanced by the superintendant-general of quarantine regarding Gibraltar yellow fever.

I have finished what appears to me necessary to mention of the late Gibraltar fever; and I trust, have satisfactorily shown the insufficiency of the grounds on which the opinion of its origin and nature, as given by Sir William Pym to the Medical Society of Cadiz, rests. I do not mean to impute motives of wilful or direct misrepresentation to the superintendant-general of quarantine; but certainly his desire to strengthen and to support favourite opinions has led him from the path of strict induction; and the Medical Society in Cadiz, instead of receiving that information they sought for, and which their judiciously drawn up queries so justly entitled them to expect, have now to form their opinions of an important and fatal disease, from mere dogmas and gratuitous assertions, contradicted by the observation of every unbiassed person in this garrison. The evil of promulgating doctrines similar to those of Sir William Pym, without the clearest evidence, cannot be too severely reprobated, especially when we reflect on the fatal effects that may result, and the thousands that may fall victims in consequence. Acting on the principles he has laid down, and believing the fever in question of foreign origin, and of a contagious nature, what are the measures pursued when it appears in any particular district or town? A cordon is instantly established, the inhabitants are shut up, all communication cut off, and those that are not fortunate enough to escape before alarm is excited, in all probability fall victims to the disease. Fortunately for us in Gibraltar, we have the means of escape in our hands; and a removal to the Neutral Ground, Europa Flats, Wind-Mill Hill, or the Bay, insures safety; but this is not the case in Cadiz, and many other large towns. There the inhabitants must patiently wait their fate, like people in a house on fire, with the doors and windows shut to prevent their getting out.

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That the fever in question may have been introduced, I do not deny; but certainly we have no proof that it was so; and I think any one who dispassionately considers the facts which I have adduced, will agree that it was not contagious. From the warmth and strong feeling, however, in which opposite parties have indulged in discussing this question, a degree of discredit attaches itself to whatever is advanced; and it cannot be denied it is difficult at times to arrive at the truth. For example, the contagionist tells us Gibraltar is one of the cleanest towns in Europe, Bath not excepted, and that there are no beggars in the place; while the advocate for local origin, on the contrary, describes it as abominably filthy, abounding in all manner of villanous smells, and with an over-crowded pauper population. Now the truth is, both these statements are partially correct; but the facts have not been fairly stated. As far as the streets and exterior of the houses are concerned, Gibraltar is clean and beautiful, without filth or beggars; but when we examine the interior of their houses, a scene of a very different nature presents itself; filth, wretchedness, and poverty are apparent at every step; and at the breaking out of the epidemic, in no town in Great Britain was there a greater proportion of poor than in Gibraltar. Matters are now perhaps better; but still there is a fair proportion of poor people who live entirely by charity; and the houses of this class are extremely dirty and ill-ventilated. Independently of this state of things being favourable to the developement of fever, the frequent occurrence of fevers so exactly resembling the late epidemic as not to be distinguished from it, is strongly corroborative of its local origin. These fevers occur at all seasons of the year; and although nice discriminators pretend to distinguish between them and the epidemic form of the disease, for my own part, I can see no difference. The leading and pathognomonic symptoms, as well as the necroscopic appearances, are the same; and, as far as I have observed, they have been equally fatal. Yet no one has ever dreamed of referring them to a foreign source, far less considering them contagious; and, in my opinion, it only requires a more general and more diffused operation of the cause giving rise to them, to produce the epidemic form of fever.

In commenting on Sir William Pym's answers, I have adhered to facts, and in almost every instance drawn my deductions from documentary evidence. The correspondence of the late Dr Hennen, which I have quoted, is particularly valuable, as it throws considerable light on the views entertained by that accurate and acute observer on the origin and nature of the late epidemic; and had he lived, many facts would have been elicited, which power, interest, and preconceived opinions now endeavour to conceal or envelope in obscurity. Before his time, as I have already mentioned (page 40,) the medical man who dared to assert, that intermittent or even remittent fevers were of indigenous growth in Gibraltar, was put down as a visionary, or one wishing to subvert the established order of things. The consequence has been, that this class of diseases seldom appears on the face of any return in the principal medical officer's office. He, however, investigated facts for himself, unbiassed by theory, unfettered by party; and we have the result of his labours communicated to us in his Medical Topography.—Yet, in opposition to the opinions therein given, Sir William Pym persists in the contrary; and in his answer to 10th query, states, in the most unqualified manner, that remittents are of rare occurrence, and intermittents never known in Gibraltar. As well might Sir William tell the people in England that it never rains in Gibraltar, or that the sun never shines. The fact is, remittents are not unfrequent here, neither are intermittents. The year 1829 was prolific in this last class; and I have this year seen several cases of the former in its most aggravated form. In the 53d Regiment, quartered at Wind-Mill Hill, four cases of genuine ague have already occurred, three in men, and one in a woman, none of whom had been outside the gates of the garrison since they arrived in November last. One has also taken place in the 94th Regiment.

In conclusion, I take leave to express a hope that Sir William Pym will, as soon as his avocations permit, favour the world with a full account of the late epidemic.

ART. III.—*A descriptive account of Frambœsia or Yaws.*

By DAVID MASON, M. R. C. S. L.

THIS disease appears to be indigenous to Africa. The inhabitants of Guinea and the warmer regions of that continent seldom fail to have the disease at some period of life,—most commonly in childhood or youth. Hilary says it is also indigenous to Arabia; but is not described by any of the Greek or Arabian physicians, except Abbas, who calls it *lepra*. The 13th chapter of Leviticus has been quoted to prove that this disease is the leprosy of the Jews; but the vague and ill-defined description of the symptoms there given, leave it doubtful what particular disease they were meant to characterise, or whether they do not apply to several distinct diseases that were considered unclean. The appearance of white hairs in the tubercle or rising, which is noticed as one diagnostic symptom of the Mosaic disease, does not occur in yaws,—at least among the African race, so far as my own observations extend; and, as the contrary has been particularly stated by several writers on yaws, I have been the more attentive to observe the circumstance, but have never been able to perceive any change from the natural black colour of the hair, although the yaw tubercles are commonly found in the arm-pits and various other places covered with hair. Sauvages describes two species of the disease, the first of which he names *Frambœsia Guineensis*, denominated by the Africans *yaw*, signifying the strawberry, from the striking resemblance of the diagnostic tubercles to that fruit. The second species, according to that nosologist, is the Frambœsia Americana, and which is called Pian by the American natives, bearing the same allusion with the African term Yaw; and the ulcer produced on the spot of inoculation is named Mama Pian. These two species seem to belong to the same disease; and the first is probably the parent of the second, by importation from Africa.

The introduction of the disease into the West India islands is unquestionably from that continent. Its origin and establishment in these islands may be distinctly traced with the importation of negroes during the early part of the slave trade, together with some other African diseases. The current of infection was long kept flowing by the commerce in slaves, and the constant arrival of infected persons among these unfortunate captives. But although that traffic has totally ceased in the British West India islands for so many years, yet the disease seems permanently established, being continually propagated by intentional or accidental inoculation.

It is a fact well ascertained, that the poison or infectious matter of yaws is never, under any circumstances, conveyed through the medium of the atmosphere. Actual contact or inoculation is essential to the production of the disease. The negroes in Jamaica are often prompted, by various motives, purposely to perform this operation; the indolent and ill-disposed, with the design of evading work for a time; and mothers inoculate their infants about the period of weaning, that they may be indulged in nursing them until their recovery; and many, from an African opinion and custom in that country, that children should undergo the disease at an early period of life. It is proved, however, by experience, that this disease is not milder in childhood, but, on the contrary, is more unmanageable, and in its progress is liable to be conjoined with other infantile disorders, which together often become dangerous and fatal, or tedious and difficult to cure. It might be inferred from the African custom of early inoculation, that few persons escape the disease in that country, and that it is considered better to undergo the long and disagreeable confinement which the cure requires, at an early age, that it may not interfere with the business and avocations of after-life. Although the disease is frequently propagated in the manner above described by intentional inoculation, yet in numerous instances the infection is doubtless communicated by accidental and unobserved contact. The yaws negroes are not proscribed by their own colour from the most familiar intercourse with the unaffected; and although it is probable that absorption of the matter does not take place through the sound skin, it is not unlikely to be unknowingly applied to a broken surface, during the careless intercourse of healthy and diseased persons. With respect to the communication of yaws through an unbroken cutaneous surface, I have made one or two experiments, by rubbing the yaws matter on such surface; but the friction produced no effect; and I infer that absorption had not taken place.

This disease is singularly different in its appearance and mode of action from any other with which the human body is affected. It has some resemblance to syphilis, being slow in progress, and only communicable by contact; but its after-effects are not so destructive, and it leaves the constitution invulnerable to future infection.

Dr Good, in his valuable systematic work, The Study of Medicine, arranges the yaws in the order *Exanthematica*; but this order of diseases, besides the eruption, is essentially preceded or accompanied by fever. It will be shown, however, hereafter, that this condition is not necessarily connected with yaws, that fever seldom occurs, and perhaps never does, except as an adventitious disorder. The class *Tubera*, to which it is referred by Sauvages, is certainly preferable, as representing more accurately the natural and uncombined character of the distemper. Several of the febrile eruptive diseases are capable of affecting the same person once only during life; but the yaws, I believe, is a solitary instance of a slow chronic disorder, producing similar constitutional changes, tending to future exemption. The time that elapses from the introduction of yaw matter until the system is affected, which may be determined by the first appearance of a yaw tubercle on the spot of inoculation, shows the early inactivity of the disease. This period extends to about three weeks, and seems nearly constant, according to the few cases in which I have performed the operation. In these cases the matter was introduced in the arm on the usual way. A dry scab succeeded, which remained stationary without scaling off, as usually happens when inoculation fails. At the end of three weeks the yaw fungus became perceptible under the scab, and soon afterwards other tubercles appeared on the body. In these instances there was no febrile action, nor any obvious constitutional disturbance. The disease was allowed to pursue its course without the aid of medicine, and the patient recovered about the usual time.

There is no foundation for Dr Darwin's observation, that the yaws is hereditary; nor is it exclusively confined to the negro or African race. Neither Europeans nor Creoles are invulnerable, although it occurs very rarely among them. These classes, and also people of colour, view the disease with peculiar disgust and aversion; and as they are not exposed to intermingle with the infected, but are careful to avoid such intercourse, nor have the same motives which so often prompt negroes to inoculate themselves and their offspring, we may find in these considerations sufficient ground for the above exemption. Something, however, may be allowed to the European constitution being less susceptible of yaws than the African.

The first appearance of this disease is usually indicated by a foul ulcer, variously and fortuitously situated, and which is ultimately ascertained to be the spot of inoculation; the skin having been previously broken either by accident or design. At an early stage this sore resembles a simple cutaneous ulcer, and only becomes suspicious by not healing with the use of common dressing. In a short time, however, it assumes a more characteristic form; the edges become spongy, and of a whitish colour, and these changes sufficiently discover the disease to an experienced observer. The poison, slow in its progress and effects, now begins to act generally on the constitution, and in most cases circular patches of minute *papulæ* appear in various parts of the skin, giving it a granular appearance. In time some of these *papulæ* enlarge, become elevated, and covered with a crust. When this scab is cautiously removed, the whitish, spongy, granular yaw is distinctly seen. The origin, early and slow progress of these papulæ, are not perceptible without the aid of a magnifier. When these tubercles are allowed to enlarge without interference, which is usually the case, they attain a considerable size; and two inches diameter is no uncommon dimension. They seem to be formed in the *cutis vera*, and do not extend into the cellular membrane, unless by some accident, or ill treatment, they degenerate into the state of ulcer. They are easily destroyed by caustic, without any mark or depression being left. For a few days the spot is distinguished by a deeper black colour than the surrounding skin, but which gradually fades into the natural and contiguous colour. I do not here allude in particular to those yaws which have been removed by caustic, because the darker shade is equally perceptible in those which have disappeared by natural absorption. The mucous tissues seem incapable of producing the yaw tubercle,—at least I do not recollect to have seen any on these surfaces. Yaws are frequently found in the angles of the mouth, but not on the lips, or in the nostrils.

It is impossible to define or describe with precision the external shape of the yaw tubercle. All of them are composed of irregular circular segments cutting each other more or less acutely, forming waving lines around the base. The summit is often flat and depressed in the middle. Those of the characteristic strawberry-shape are considerably elevated, and exhibit an irregular granular surface. In some parts, where the tubercles are numerous, they unite and form connected clusters. Occasionally the tubercles appear in the form of a ring, with a depression of sound skin in the middle, or the elevation is less entire, only forming a circular segment. But whatever figure these risings assume, they all discharge the same sort of matter, which is condensed into a similar scab, and which, being cautiously removed, shows the dingy granular surface underneath.

Among healthy negroes, exposed to the simple action of the yaws, the constitution at first indicates little apparent disturbance. The most common symptoms, although by no means constant, in the early stage are a tinging or itching sensation in the skin, and slight pains in the joints. The mother yaw, or that caused by inoculation, which generally ulcerates, sometimes gives considerable pain. As the disease advances, and the yaws become large and numerous, the constitution shows more evident signs of disorder. The previous plump and healthy look is changed to marks of debility, the patients becoming thin and emaciated, but never in ordinary circumstances causing confinement; and under proper regimen and care the patients preserve throughout their natural liveliness and ease.

Although the strawberry tubercle discharges a considerable quantity of serous matter, which rapidly encrusts the surface, yet this morbid secretion produces no immediate action on the surrounding skin, which is often smeared with it from frequent breaking of the scab. This matter will create a yaw wherever it comes in contact with broken cuticle,—a circumstance which seems to corroborate a former observation, that the yaw virus is not absorbed by the sound skin; nevertheless, it is proper and salutary to wash off this condensed matter wherever it adheres to the skin. There yaw tubercles will remain long stationary if undisturbed by dressing. I have already observed that they are speedily obliterated by caustic, and I have reason to conjecture, from some casual observations, and the inactivity of the infectious matter, that the mother yaw might not only be healed by this means, but the constitutional disease prevented by its timely application to the source of infection.

There seems to be an inaccuracy in Dr Thomson's description of the yaws, in the Edinburgh Medical Journal, (Vol. xv. p. 323,) where he states that a healthy yaw is red. It is probable he had cursorily seen the yaws after the crusts had been rubbed off by the nurse in the rough way which is commonly practised, and which produces an oozing of blood on the surface. In the natural state of the tubercle the capillary vessels discharge a whitish matter, which gives it a pale white colour. In fact, Dr Thomson's whole account appears to me deficient in accuracy,—at least it is at variance in many respects with my own observations. The most healthy yaw, according to his statement, is that which I have commonly found to arise in weakly and diseased habits; and he would lead us to expect an eruptive fever as an invariable precursor of yaws, which I have already stated, and will have again an opportunity of showing, is far from being the case; indeed it occurs so seldom, according to my experience, that I have been induced to assign other causes for its occasional excitement. In confirmation of this view, I shall here relate two cases which seem to prove, that during the slow progress of the yaw infection the patient may be afflicted with other diseases, which may accelerate the eruption of yaws, and thereby produce a combination of symptoms which have been inaccurately assigned as the exclusive result of the latter disease.

A young negro male, about 25 years of age, was seized with severe pyrexia and the common symptoms of pleurisy. He was treated by blood-letting, purging, and the usual remedies for that disease. In a few days these symptoms were subdued, and on their abatement an uncommonly numerous eruption of yaw tubercles appeared over the whole body. After recovering from the febrile attack many of the tubercles disappeared, others enlarged, and the disease pursued its course in the usual way, and terminated about the period of twelve months.

A young mulatto woman, about 30 years of age, was affected with pain in the joints, accompanied with paroxysms of fever. At the end of three days a general eruption, resembling chicken-pox, appeared over the body, with papular and vesicular pustules intermixed, attended with a burning and itching sensation and pains in the joints. In about a month after the breaking out of this efflorescence, some of the pustules had enlarged, and were covered with the whitish crust, which sufficiently distinguished the disease that remained. After this period many characteristic yaw tubercles were formed; but in this case they were broader and flatter than usual, and were throughout mixed with the pustular eruption long after the vesicles had scaled off. In other respects the disease ran the usual course, and was cured without the aid of medicine.

The first of these cases seems to be sufficiently marked by the symptoms of pleurisy, brought on by causes which we cannot always discover, and most probably after the yaws was established in the system; perhaps about the usual time of its developement, or its tardy action may have been accelerated by the more violent and rapid effects of the acute disease.

The second case I considered a combination of yaws and chicken-pox; and the combined operation of these two diseases caused at first some anomaly in the former malady; but, however much it might be modified or somewhat altered from its usual appearance by the simultaneous action of an acute disease, the latter had no effect in increasing its activity or weakening the durable hold which it always takes of the constitution. The chicken-pox I have no doubt ran its course and disappeared, leaving its companion to effect its termination in a less rapid way. It would be out of place to enter on any discussion concerning the simultaneous existence of two diseased actions in the animal economy, wholly distinct and uninfluencing each other; such a twofold condition is scarcely conceivable in the same part. But it is not equally difficult to understand that two such agents may mutually modify each other when their operations are incidentally determined to the same organ. But leaving these points undecided, I do not think it inconsistent with analogy to infer that the eruptions of chicken-pox and yaws may occupy different parts of the skin at the same time, and may intermingle without coming in contact with each other. The vaccine disease has no apparent influence on the yaws. It is not uncommon to vaccinate both children and adults during all stages of the yaws, and I have never observed that these two diseases interrupt or disturb each other's progress.

To return to the description of the yaw tubercles, I proceed to observe, that the eruption continues, more or less numerous, until the disease is entirely extinguished. They vary in number, occasionally increasing and diminishing without any apparent cause. Towards the termination of the disease, the large tubercles often completely disappear; and a kind of secondary eruption, few in number, again takes place, after a considerable lapse of time. During this interval, however, of apparent recovery, it seldom happens that the skin is free from clusters of the small papular yaw, and these remain visible, sometimes for weeks or even months, after every return of the larger yaws has ceased. This wavering stage of the disorder, when the constitution has nearly undergone the extraordinary change which renders it for ever after impregnable to the infection, is somewhat curious. It seems to show the power of long habit, by which morbid actions maintain their ground against the salutary efforts of nature. I could mention several examples of these secondary yaws reappearing, and obstinately continuing, if undisturbed, for twelve months, after the cure of the constitutional symptoms to all appearance had been complete. It may be thought, perhaps, that these yaws were the result of fresh inoculation. This, however, is not verified by any facts from experience, as far as I know; and their occurrence has taken place, in some instances, within my own knowledge, under circumstances in which I considered it next to impossible that any new infection could have been received.

During the long and tedious progress of the yaws, it is not uncommon for such patients to be incidentally affected with remittent or continued fevers. The peculiar action of these diseases on the surface of the body, by suppressing the perspiration, dries up the yaw tubercles with sudden and remarkable rapidity. Fevers under such circumstances are usually severe and dangerous; but when the fever abates, and the natural perspiration is restored, the yaws reappear, and proceed with their ordinary inactivity. A state of increased debility, with a tendency to dropsy, however, is frequently the result of the morbid influence of both diseases.

The foregoing is a brief, but, I believe, accurate account of the symptoms and progress of the yaws, in a mild and unmixed form, as it commonly appears among healthy negroes, who are treated with proper care, during the long period of its continuance. But it is also necessary to view the disease variously aggravated and modified by natural constitutional derangement, accidental exposure, or artificial mismanagement and ill treatment. These circumstances have great influence both in altering the external form of the eruption, and creating new and formidable symptoms, which otherwise would have had no existence, and which, I conceive, have led to much erroneous and contradictory description.

Since the disease is not communicable by effluvia, it is probable that any variable quantity of morbid matter introduced by inoculation is immaterial, and that neither diminution nor aggravation of symptoms is created by any irregularity in that circumstance. We must look, therefore, for other causes, to account for the alteration and exasperation of symptoms which occasionally occur in the yaws, and which have given it a character far more malignant and destructive than can be assigned to it from the train of symptoms already described.

Dr Good, in his Study of Medicine, has divided the yaws into two varieties, the African and American; the first seems to be the variety to which the preceding observations refer, although differing in some essential particulars from his account of it. The second variety probably includes the class of symptoms subsequently to be detailed. There does not appear to be any good foundation for this distinction, as we have no reason to suppose that the African and American yaws materially differ under equal conditions in their common character. The very names seem to prove that they are the same disease, and distinguished by the same species of tubercle. Presuming, then, that the disease is of a uniform nature, and that the malignant and anomalous symptoms which occasionally present themselves are the result of internal constitutional, or accidental external, circumstances, I shall endeavour to explain the operation of these causes and symptoms from the suggestions of experience and observation.

With respect to constitutional or hereditary predisposition existing in a latent state in the animal system, especially of such a nature as may be brought into action by diminishing the powers of life, I have reason to conclude, that such morbid excitement is occasionally the result of yaws, by which is meant, that the predisposition would have remained dormant, perhaps for years, had it not been roused by this incidental disturbance. The yaws is essentially of a slow chronic and debilitating nature, as has been already shown even in its mildest form; and these effects are considerably augmented, by neglecting the means which might counteract, or diminish their influence, such as food and cleanliness; and under these unfavourable circumstances, scrofulous affections are sometimes brought into premature and exasperated operation. In these cases, scrofulous ulcers make their appearance in different parts; the yaw tubercles lose their prominence, become flat, and spread considerably in the skin, discharging a thin ichorous fluid, which corrodes the cellular substance, and when healed, leave large and indelible marks. The whole of the yaw tubercles, however, are not thus corrupted; but all of them are changed in some degree from the usual healthy look, become more scabby, and lose their characteristic shape. It most commonly happens too, in course of time, that the bones become more or less affected, sometimes with simple enlargement of the bones of the legs, and fore-arm; at other times, ulcers extend to the periosteum, and the subjacent bone soon becomes extensively carious. These affections of the bones, although they accompany, do not terminate with the yaws. They often continue for years after the eruption of yaws has wholly disappeared, and in some malignant cases gradually increase until the death of the miserable sufferer. Another train of symptoms is sometimes, though less frequently, conjoined. The membranes of the nasal passages ulcerate and corrode the adjacent bones, which grow carious; and frightful incurable ulcers are established in the nose, palate, and throat.

Taking into consideration the origin and close connection of all these formidable symptoms with the well known disease of yaws, it is not wonderful that they should have been so frequently considered and described as a part and remnant of that disease. But I would observe first, that these unusual symptoms, exclusive of the degenerated state of the yaw tubercles, are characteristic of diseases among negroes who have never undergone the yaws, or who have been affected with that disease in early life, long before the commencement of the foregoing train of symptoms. In the first instance it is manifest, that they could have no connection with the yaws; and in the last, it is perhaps going too far to attribute a series of symptoms and effects, as the relicts of a disease that to all appearance had been long cured. Besides the vitiated features which are stamped on the yaws, by what I consider constitutional diathesis, or the combination of hereditary diseases with it, other cases occur, from obvious mismanagement and neglect, which produce no less considerable changes on the state of the yaw tubercle and the general health of the individual. Inattention to the supply of proper food, and negligence in the necessary clothing and cleanliness, produce their usual effects on persons affected with yaws, and co-operating with the morbid tendency of the disease, their united influence acts with twofold violence on the constitution. Derangement in the organs of digestion and dropsical debility speedily ensue. The countenance is bloated, the skin dry and anasarcous; and white tongue, swelled abdomen, and dropsical effusion in the lower extremities, are the common symptoms of this state of ill health. The yawey eruption too is not slow in manifesting the influence of these general elements of disease. The elevated yaw tubercles become flat and spreading, discharging an ichorous matter, which ulcerates the skin, and in this way the numerous ulcerations often meet and form extensive erosions. The difficulty and trouble of dressing these diffuse cutaneous erosions causes them to be neglected altogether, or very imperfectly managed. Hence children, among whom it chiefly occurs, are worn down with pain and debility; and if fever accidentally ensues during this feeble and miserable state, it usually proves fatal. The altered appearance and acrimony of the yaw tubercles is so unquestionably brought on by constitutional debility and ill health, arising from other causes than the yaws, that I have never seen this condition of the tubercles among negroes enjoying general good health. An important practical inference may be therefore drawn from the circumstance, regarding the treatment in this disease,—that it requires an ample supply of nourishing diet, frequent bathing, and moderate or equable warmth.

I have good reason to conclude, that no specific remedy has yet been discovered for the speedy and effectual cure of yaws. In ordinary circumstances, its action is chiefly on the surface. The only effects on the constitution internally are not very observable, the symptoms being obscure and almost insensible; and it is probable, that the long continuance of the cutaneous eruptions ultimately produces that state of debility which invariably succeeds. Reasoning on this pathological view of the disorder, we would be led to prescribe diaphoretics and tonics, and the usual regimen that promotes and supports the operation of such medicines; and, on the whole, I think this plan the best. By this treatment, however, a rapid cure cannot be expected; for such is the firm and irresistible hold which is taken of the system by this singular distemper, that a considerable time must elapse, under any circumstances or management, before it can be overcome. But perhaps no disease has more excited the ingenuity of quackery than the yaws. Its slow progress and inactive character present a favourable field for the exercise of ignorance and empiricism; and the occasional and fortuitous disappearance of the larger tubercles furnished a show of success to numerous nostrums, since it has happened that various materials of little or no value have been extolled as effecting a rapid and infallible cure. Practical experience enables me to decide on the virtues of several of these remedies, both simple and compound; and I have found them altogether undeserving of the praise so confidently bestowed, and either utterly useless, or quite insufficient to extinguish the disease at an earlier period than is effected by proper regimen alone. Many of these nostrums, suggested and advertised by unprofessional persons, are from the vegetable class, and seldom recommended in the cure of other diseases. Professional men have usually employed known officinal medicines; and of these, mercury, antimony, and sulphur, and their compounds, hold the first place. The former of these and some of its combinations are certainly of no decided benefit; and I am rather inclined to condemn them as prejudicial, when given as alteratives for a time. Antimonials, and particularly tartar emetic in small doses, may be of some use; but their effects on the peculiar morbid action of the yaws are not very obvious. Sulphur and sulphurets are not more advantageous, and have probably been suggested from their known efficacy in curing some other cutaneous diseases of a very different nature.

The most safe and judicious treatment consists, first, in frequent ablution; and for this purpose the tepid bath, if regularly and cautiously managed, is best. But these requisites cannot be fulfilled without a proper bathing apparatus and close rooms, appendages which cannot always be commanded for negroes; and in the hospitals exclusively assigned for the reception of such cases, the patients are often so numerous, that the constant use of the warm-bath would become too laborious for the attendants to employ it with patience and regularity. But a safe and convenient substitute, although perhaps not so effectual, is found in a common bathing place in the open air, and the immersion practised after the water has been warmed by the sun. This tepid ablution, besides its effects in promoting insensible perspiration, cleanses the skin from the viscid matter discharged from the tubercles which adheres to the adjacent cuticle; and although in a healthy state it does not corrode the skin, its removal cannot fail to be salutary. Cleanliness in regard to clothing will also contribute to preserve the good effects of the above method of purification, and therefore becomes an object of attention. The medical treatment connected with these mechanical operations is simple, and merely auxiliary. A diet drink of sarsaparilla decoction, in which is dissolved a small proportion of tartar emetic, may be advantageously used in the earlier stage of the disorder. At a later period, when debility prevails, it may be superseded by more appropriate remedies. When the eruption is completed, and its action on the system begins to be fully exerted, tonic medicines, particularly preparations of iron, come more properly to be recommended; but these will be of little avail, unless supported by full diet, with a liberal allowance of animal food. Negligence with respect to sustenance, and the co-operation of bathing and cleanliness, lays the foundation of that anasarcous debility and functional derangement of the digestive organs which has been already noticed as often altering the general appearance of the tubercles, and aggravating all the usual symptoms of a disease, which under other circumstances and better management proceeds in its course mildly and without danger. This state of aggravated debility, when the constitution is not too far undermined, may be removed by frequent doses of calomel purgatives, and the use of chalybeates, aided by proper nourishment, suited to the impaired condition of the organs. The slow but safe treatment above described, nearly confined to diet and regimen, is the best I am able to suggest, after much experience and consideration, and many trials of popular remedies, in the virtues of which, however much extolled, I can place no confidence.

I come now to make a few observations on topical remedies, which I am inclined to think will be found far more effectual than has hitherto been supposed. Of these the most speedy [and effectual is the nitrate of silver. The rapid destruction of the crab yaw, which is effected by touching it freely with this preparation, induced me to extend its application to those old standing secondary yaws, which often linger in the skin long after all signs of constitutional disease have ceased; and its effect on them was still more immediate and complete, one or two applications being commonly sufficient to remove the largest yaw. This speedy effect is not accomplished by the mere local act of the caustic on the surface of the tubercle; but a stimulus seems to be given at the same time to the energy of absorption. Before the caustic is applied, it is necessary to remove the livid scab which encrusts the surface of the yaw, to bring the escharotic in contact with its spongy granular substance. The yaws so destroyed seldom reappear on the same spot; but the others, which continue to arise for a short time in various other parts, are finally extirpated by the same means, without any other consequence than the transitory pain which the caustic occasions. In this manner I have repeatedly eradicated this morbid cutaneous action, after it had existed more or less in the system for the period of two years. Reckoning from the commencement of the constitutional infection, and during the last year of that long term, it may be fairly considered as holding its ground in a secondary or habitual state; and though it is not easy to explain this morbid condition, facts seem to prove that its power in the system is feeble and exhausted, local in its operation, and easily subdued by proper artificial means; but when left undisturbed, it may continue long without being overcome by the efforts of nature.

I may here mention the case of a young negro girl, about ten years of age, who, in the early stage of the yaws, took a compound pill of calomel, camphor, and sulphuret of antimony, with decoction of mesereon; and these medicines were continued daily for nearly four months, having been prescribed when the first eruption of yaws took place. At the end of that time the medicine was discontinued, without having produced salivation; and although the yaw tubercles were considerably reduced in number, a diminution usual after the lapse of some time, they did not seem to be much on the wane. The local treatment by caustic was then adopted, and effected a complete cure in two weeks. This girl now, after an interval of twelve years, has never had crab yaws.

The power of the nitrate of silver in suppressing the yaw tubercles in the latter periods of their progress, and speedily terminating the diseased action, suggested the trial of it in the earliest stage. For this purpose, I selected the case of a healthy negro boy, about eight or nine years of age. He had first a small sore on the upper part of the foot, which was surrounded by a papular eruption. The sore was healed by the application of lunar caustic; but soon afterwards numerous patches, mostly circular, of minute papular specks, broke out in different parts of the body; and under the chin a distinct granular yaw tubercle was formed, which, in fact, is merely a cluster of these contiguous papulæ enlarged and united. This yaw was immediately destroyed by caustic. Another arose in the neck, and was destroyed in the same manner. During this time the papular eruption was sponged, first, with a solution of muriate of mercury, and afterwards with a solution of nitrate of silver. The boy had no fever, his general health was good, and he complained of no pain or uneasiness in any way. At the end of some months, the number of which my notes do not mention, nor do I accurately remember, the papulæ disappeared, no other tubercular yaw being formed, and the boy resumed his occupation as a domestic. To determine whether or not the cure was complete, and the constitution secured from future infection, I sometime afterwards inoculated the boy with yaws in both arms. About three weeks thereafter, yaws began to appear, first about the neck, followed by others in various parts of the body, accompanied by clusters of the papular yaw. The larger tubercles were destroyed by caustic before they attained any magnitude. They were throughout few in number, and were constantly reduced almost as soon as formed. This practice was steadily continued for upwards of twelve months before the disease was entirely extinguished. The boy in other respects preserved perfect health during the whole period, and no internal medicine was employed. Not being distressed by the pain and uneasiness of the yaw tubercles, nor reduced by the great discharge of viscid matter, when they are large and numerous, he certainly suffered little of that debility which is the common effect of the disease.

It may be observed in this case, that the first infection was incomplete, in not affording that permanent constitutional protection which is the usual result, the disease being reproduced by subsequent inoculation. One advantage, however, was manifest. The disease was divested of its disgusting appearance, and the patient relieved from those painful sensations which so frequently accompany the eruption of numerous yaw tubercles. Nor did their suppression affect the general health; and, if it did not expedite, I have no reason to believe that it retarded the cure; and I can further add, that after the lapse of two years since the boy's recovery, no crab yaws have ever appeared on his feet.

Although this single case does not furnish a very satisfactory example of the benefit of topical practice exclusively, it seems, notwithstanding, worthy of farther trial carried on in conjunction with the general plan already recommended. I have also some reason to believe, that the early application of caustic to the mother yaw or that of inoculation, before the constitution is tainted, may speedily effect a cure, or prevent the absorption of the poison,—an object of no small consequence in many cases, although by so doing the future susceptibility may remain.

I can now only make a few remarks on that residue of the disease called the crab yaw, an awkward and not very exact term, denoting some fanciful similitude between the motion of the crustaceous insect and that of the barefooted sufferers from this complaint. It might more properly be denominated in familiar language the chronic creeple, or cripple yaw, from the excessive lameness it occasions in walking. The tubercles are confined almost entirely to the soles of the feet. Like the other yaws, they seem to arise from the true skin; but, being confined during their growth by the hardened cuticle, that resistance, together with the pressure in walking, creates intolerable pain and inability to move, unless in the singular and awkward way already mentioned. As the tubercle enlarges, the upper covering gives way, and they appear above the surface in the shape of granular yaws. They are easily removed by paring the hardened skin around, and cutting off the projecting tubercle as near the root as possible, and immediately touching it freely with caustic. But this operation, although speedy and efficient in destroying these chronic yaws when they are formed, does not prevent the growth of others at different places and after various intervals of time. It is difficult to account for the obstinacy with which this remnant of the yaw adheres to the soles of the feet for such an indefinite period. Its formation appears to be engendered and fostered by the hardened skin in some negroes; while in others, who are more cleanly and careful of their feet, the complaint is more rare and of shorter continuance. The negroes employ escharotics to remove the crab yaw, but so injudiciously and imperfectly, that much time is required to effect their purpose.

An African practice, however, still in use among the ignorant negro empirics, ought not to pass unnoticed. It is always attended with danger, and in some instances within my own knowledge has proved fatal. It consists in immersing the feet in a medicated hot bath, composed of a watery decoction of various herbs, and which, by means of a slow fire, is kept constantly near the boiling temperature. In this state the patient's feet, closely wrapt round with woollen cloth, are retained in it for the period of nine days with barbarous perseverance, often in spite of the cries and supplications of some of the more sensitive sufferers, while others undergo the process with callous indifference. These are instances, as I observed above, of the former class of unfortunate individuals who have expired on the spot. The effects of this dangerous practice on the local affection tend to remove the hardened skin from the soles of the feet, and with it most probably the crab yaw tubercles; but of the permanency of the cure I am not sufficiently informed.

Art. IV.—*Remarks on the common Cachexia, or Leucophlegmasia, called Mal d'Estomac in the colonies, and its kindred affections, as Dropsy, &c.* By Dr J. Hancock, F. Med. Bot. Soc. Cor. Memb. Zool. Soc. F. Phil. Soc. Brit. Guiana, Hon. Memb. Soc. Arts, Edin. &c.

THE term of dirt-eating, used in the English colonies to denote this complaint, it would be vain to combat, although, without doubt, it arises out of ignorance, and from mistaking the cause for the effect. The French give it the above much more appropriate name, being somewhat significant of its nature.

We may regard this disease as a perfect example of *atrophia* in its most genuine form, or of cachexia, poverty of blood and debility, unattended by fever, cough, or other disturbance of the corporeal functions,—and therefore we have, in this instance, a specific name, (Mal d'Estomac,) formed merely on account of the perfection of the symptoms, as if it were a malady *sui generis*, distinct from the *atrophia* of Europe; an assumption, however, which appears to me to be unfounded.

Its causes are want of due nourishment of the body, or inability to assimilate the food into chyle; exposure to wet and cold, and whatever tends to weaken the body; worms preventing the nourishing effects of food; depressing passions, &c.

The primary evil constituting this disease is a cacochymia, or depraved state of the fluids, depending on a faulty chylification, owing to various causes as above cited, and to the defect of the agents denominated non-naturals, viz. healthy air, rest, regimen, &c.

That such is its origin is proved by its rarely occurring, excepting in those exposed to the causes just noticed.

Mr Dalton's man, Alfred, from Tiger Island, who died in a cachexy, or *atrophia*, on being opened, showed no deviations from the healthy structure in any of the viscera, except a slight enlargement of the glands of the mesentery, about two ounces of water in the thorax on each side, a slight effusion in the cavity of the abdomen, and the spleen shrunk, or smaller and more dense than natural, affording a great contrast with its state in yellow fever. The latter phenomenon seems in some degree corroborative of the opinion we have heard advanced respecting the economy and uses of this organ in the system—*i. e.* as a reservoir or recipient for the superfluous blood. In the case of yellow fever there is usually a superabundance of blood in the system, but in the former, a paucity for the most part—at least of good blood; for the disease appears to me to consist more in a defect in the quality (depraved state of the humours) than in any deficiency in respect to quantity. The fluids, indeed, in dropsies are too abundant, but watery, and extravasated out of their proper channels, partly from want of power in the exhalent arteries, and relaxation of the cellular membranes. The blood having lost its vivifying principle, fails to afford its wonted stimulus or pabulum to the solids.

If *transfusion* could avail in any disorder, it ought to do so in *mal d'estomac* and in dropsical habits, or what is best designated by the term *leucophlegmasia.*

The case above cited was one of true cachexia without perceptible organic derangement, except very slight mesenteric obstruction. The symptoms were precisely the same as in *mal d'estomac* in general; viz. paleness of the inside of the under lips and gums, bad colour of the skin, sluggishness, and extreme debility.

The nature and causes of this disease being misunderstood, the patient is usually locked up to prevent his getting access to any absorbent substances which the stomach craves; and in many cases he is diligently purged, with the view of expelling any absorbent substances from the bowels. In this manner he is totally deprived of the little strength he may have remaining, by practitioners who consider the clay, calcareous substances, &c. to be the cause of the disease, of which it is rather an effect, this propensity depending on the morbid state of the stomach, and the vitiated secretion of the gastric juice. We observe the same propensities in chlorotic females, both here and in northern climates. The Africans are, moreover, in the habit of eating clay, and several earthy substances, in their own country, and ever with impunity, as they constantly assert; and we know that the Parianos, Otomacos, and certain tribes of South America, do the same. I have heard some of the more intelligent Africans express their surprise that the practice should prove injurious in the colonies; for they are led to believe in this fancy by those who ought to know better.

The proper mode of cure, in place of confinement, consists in giving the patient unrestrained freedom, ease, and amusement with his companions; for despondency and inactivity is a prevailing symptom, and the nervous system is primarily implicated in the production of this disease.

The bowels should be kept free by gentle purgatives, not of a debilitating but of a cordial nature. Rhubarb may be considered the best, with a little ginger, and magnesia as an antacid.*

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* In this disease, and various forms of dropsy, I have found the *Coosia*, or bitter cucumber of Guiana, a useful remedy given once or twice a-week, where a potent hydrogogue was required.

To amend the impoverished state of the blood, and give tone and energy to the system, there is little doubt that preparations of iron should hold the first rank.

The ferruginous impregnation of the water from the iron cistern brought out in 1824 or 1825 was thought, according to the report of the judicious manager, Mr Keane, to be the restorative agent and chief cause of the great amelioration which ensued in the health of the negroes at Plantation Sophienburgh, on Tiger Island. From his account, it was mainly instrumental in the recoveries, as before this period there were many *mal d'estomac* patients on the estate, and but few or none afterwards.

This appears to me to afford a suggestion capable of great practical utility in the colonies or elsewhere. There is no doubt that the water contains a sufficiency of carbonic acid to enable it to dissolve a salutary portion of the iron; and if the slight chalybeate solution thus formed be really beneficial in the removal of *mal d'estomac*, it is a method both prophylactic and sanative, of which planters might easily avail themselves.

Chalybeates, however, should be joined with other tonics, as the Peruvian bark, though I should prefer the Angostura bark, from a thorough experience of its efficacy in dropsy, which we may consider as almost a synonymous term, or as nearly identical, for the essential conditions of each are common to both. Mercury will also be found of great avail in these cases, if properly administered; but in the common way of using this mineral, it will often prove hurtful, by acting with severity on the bowels. To obtain the most beneficial results it should be given in minute doses, combined with opium, as calomel and opium half a grain of each every night; and this course should be continued till some degree of salivation is excited; and it may be alternated with the tincture of muriate of iron, light nutritive diet, and exercise suited to the strength of the patient. With this course should be joined the use of sarsa and the vapour bath.

It is probable that arsenic might here be used with advantage; but I have never tried it in these cases. *

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* Certain empirical remedies are occasionally found to have great efficacy, and deserve more attention than has yet been bestowed in discovering the nature of their pharmaceutical composition. Dr Paris has justly observed, that they not unfrequently afford instances of some of our best compounds, whilst the modern mania, for simplicity in prescribing, has robbed us of many of the more efficient remedies formerly in use as officinals. Some twenty years ago a certain nostrum was used in the colonies, and with great success in *Mal d'Estomac*, as reported by certain practitioners. It was sent home to some eminent chemist for analysis, and reported to contain *arsenic*. This was not improbable, although I have learnt to place but little confidence in the pretended examination of quack remedies. We see in several late works a display or professed disclosure of such secrets, and in which the components are mentioned without the least doubt as being indicated by chemical analysis. The reports we have hitherto seen published in some magazines, and repeated in the

The cold bath might be serviceable, but should be used with great caution; for whatever may be said of the tonic effects of cold water, it will be found that a continuance for some time in the cold bath will have a direct debilitating effect on the body, and especially so on persons already much reduced in strength. In cases where its tonic effects are required, it should only be used by dashing a pailful or two over the body, to act as a sudden shock, followed immediately by friction with a coarse towel, and by this means a glowing warmth be excited, and the vessels brought into action.

Cold water *used in this way* is extensively applicable to useful purposes. The writer has lately found it to be the most effectual remedy in palsy, from experience in his own person, after the usual remedies had been found useless.

The importance of this remedy is seldom, I presume, duly appreciated. It is capable of fulfilling two of the most important, and, at the same time, opposite indications in medicine, according to the mode of its application. When gradual and continued, it proves one of the most powerful sedatives, lowering the temperature of the body, abating febrile excitement,

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Pharmacologia of Dr Paris, Rennie's and Gray's Supplements, &c. are little worthy of regard; they have for the most part been got up merely under a show of science.

The old nostrum, for instance, called "Spilsbury's drops," is said in these reports to contain two drachms of corrosive sublimate to a pint of the menstruum; but several samples I have examined showed no traces of this mineral. One of these I bought expressly for examination from the proprietors in Soho Square, but the sense of taste alone would show this absurdity. If it contained sublimate, although but a twentieth part so much as alleged, it would even then be sensible on the tongue. The same writers affirm, that a composition called "Norris's drops" is nothing but a solution of tartrate of antimony in rectified spirits of wine. It is rather strange, that, after passing review under so many learned authors, it should not be discovered that such a solution is utterly impossible—not the smallest sensible portion being soluble in alcohol.

In Good's Studies of Medicine, 3d *edition*, by Cooper, I observe another equally scientific prescription for toothach, viz. "*ʒij.* of alum to be *dissolved in* 7 of the nitrous spirit of ether, and the solution applied to the tooth." This was promulgated at a meeting of the *Med. Soc.* see Vol. i. p. 58. If the pain was not to cease till the alum be dissolved, I should pity the poor sufferer; yet Dr Good, as if well acquainted with the remedy, speaks very highly of it. (I observe by the way, he has mistaken the vermifuge bark tree, *Geoffroya*, for a species of palm; *Areca oleracea*, Linn.)

I could give many other instances; but I think the present may serve for a sample of the *Alchemia triumphalis* of modern science.

But with respect to such false reports, some will exclaim, "It is right to put people on their guard against quack medicines." Now it has no such effect, but is liable to produce the most fatal results. Gentlemen who give publicity to such misstatements are not aware they are entailing much greater evils on society than could arise from all the quackery which they are desirous to defeat. They should consider that the more saleable nostrums are prepared by many other persons besides the proprietors, and that the prescriptions thus published will be followed by numerous imitators. Let us suppose, then, that a person who has been in the habit of taking large doses of *Spilsbury's drops*, gets a bottle of that prepared with *ʒij.* of sublimate to a pint of menstruum, and takes his usual dose, the result, it is plain, might prove fatal.

deriving the blood from the capillaries of the surface, retarding the action of the arteries, so as frequently to bring a violent fever to a summary termination. On the other hand, when suddenly dashed upon the naked body, it excites the vital powers, rousing the nervous energy in the most extraordinary manner, so that a person under the most soporific and deathlike effects of opium, and absolutely unsusceptible of pain, will be instantly brought to his senses by a pailful of cold water dashed over the whole surface of the naked body, and that too, as I have often witnessed, after all other means have failed to rouse the patient, and in some instances even after respiration had ceased, or been interrupted for some minutes. Such was the case of a child of my own, and such that of a black man on Plantation Coffee Grove, as Mr Campbell, of Sparta Estate, Essequebo, can testify. Though the latter patient, after taking more than half a pint of laudanum, was incapable of excitement by the puncture of needles, or a live coal from the fire applied to the skin, he was restored by the affusion of cold water, alternately with pepper frictions, and perfectly recovered.

To return, however, from this digression. In *mal d'estomac* the treatment indicated consists of a restorative diet, tonics and cordials, a portion of wine or other spirits, together with sarsaparilla, especially bark, tincture of steel, and opiates.

In this disease an obstruction to the circulating fluids is said to prevail; but it ought to be considered, I conceive, that such obstruction does not arise from any positive impediment to the circulation, but from defective stimulus in the blood, relaxation, and want of energy in the vessels to propel the blood to the extreme parts, and to the surface. Hence the pallid, sickly, leucophlegmatic countenance, and morbid appearance of the whole body.

I have, moreover, remarked a singular appearance of the skin in some of the lighter casts afflicted with the complaint, viz. an immense number of small blood-vessels being distributed immediately under the skin. On puncturing them, a vapid and watery sanies escapes, and a small vessel being emptied in this way, does not fill again immediately. I have often observed the same appearance about the ancles, especially in gouty habits.

Gentle frictions frequently repeated over the whole body, contribute to restore the healthy action, and return the extravasated fluids into their proper courses. If performed with the hand, anointed with oil or tallow, it tends to soften the skin, open the exhalent vessels, at the same time to excite the nervous energy, stimulate the fibres, and promote the too languid circulation of the blood. We often find the skin rough and obstructed; in this case we should resort to the vapour-bath, impregnated with aromatic herbs.

To ascertain if any indurated earthy substances are passed, a smart dose of castor oil may be given, and the feces inspected; but I am little inclined to insist on this measure; for, having opened the bodies of several who died of this disease, I never found the smallest evidence of such indurated masses of clay and dirt, as we hear so frequently spoken of, although it doubtless might occur at times when taken in excess.

The improvement made on the man Horace of Devonshire Castle, by blood-letting, which was done for inflammation of the chest, was remarkable. He had been long a mal d'estomac subject, and for a considerable time dropsical. After the bleeding he was able to work in the field; the inside of the lips was no longer pale, though quite so before, and he had no œdamatous swelling afterward. He was bled so copiously that he fainted and fell on the floor on going to lie down after the bleeding. The blood was thin and watery, with but slight if any appearance of the buffy coat. *

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* This case should not be confounded with a disorder less frequently met with, which may probably be termed inflammatory or acute dropsy. This usually comes on suddenly from hard drinking, together with exposure to cold and moisture. Great obstruction seems to prevail, and the pulse is sometimes full. It seems a true phlogosis, although seldom attended with fever; the blood drawn will, in a large proportion, turn to a strong buff.

Amongst many others, I may here notice a rather remarkable case of this sort which fell under my observation in Essequebo in 1827.—J. Fraser, a young man of plethoric habit, had lived intemperately, and been much exposed in the rainy season, when a swelling commenced first at the ancles, and gradually extended over the whole body, not only the cellular membrane, but the abdomen and chest were evidently occupied by an aqueous deposite—the swellings were œdematous—some water was let off by scarifying; various remedies were prescribed, but none of them had the smallest apparent effect to abate the swellings; no pain but a dyspnœa and weakness were complained of, yet the plethoric and sanguine habit of the patient plainly indicated the necessity of depletion, and he was freely bled five or six times within the first fortnight or three weeks, to the complete reduction of the swelling in every part of the body. The blood exhibited the buffy coat; although this was not made a pretext for further depletion.

The patient was chiefly under the care of Dr Macredie, who happened then to reside on the same estate. This gentleman is now in Edinburgh, and could relate the particulars much better than myself; I only give it from recollection. I was told the patient went afterwards amongst the islands and died,—whether he took a relapse, (the most probable,) or what complaint killed him, I was not informed.

The best method of treatment in such cases appears, so far as my experience goes, to be depletion, then mercury, antimony, the vapour-bath, and frictions over the surface of the body.

Even in the most adynamical fevers, as in those emulating the plague, bleeding is proper at the onset. But then the use of tonics, and the most efficient stimulant sudorifics and alexiterics, (wine and opium especially,) are at the same time required, together with the vapour bath and frictions.—The whole should be synchronous, in order to rouse the vital powers at the time a vein is opened, to give freedom to the circulation. I presume that the same might, in the plague itself, prove the most successful. To open a vein and bleed with judgment, never appeared to me to be hurtful on the invasion of any fever.

The operation was repeated in my absence once or twice beyond what I had directed. I must, therefore, yield the merit of the cure to the hospital attendant, a very attentive old man, known by the name of *Dr Dick.*

This case, and its mode of treatment, affords an instance of the co-existence of two disorders of opposite natures, or else of the subversion of one disease by the intervention of another, and seems to offer a contradiction, or an exception at least, to our pathology, in respect to those diseases in which we are accustomed to regard all excessive evacuations, of blood especially, as directly pernicious.

But although I have long entertained a conjecture that we have been wrong in this particular, I never ventured to put the matter to the test of experiment till the occurrence of the above case, in which the urgency of the inflammatory symptoms overruled all considerations arising out of the previous state of the patient.

I am doubtful, however, how far the cure of the original disease, (the leucophlegmasia,) is to be attributed to depletion, or in what degree it resulted from a new action induced in the system by the occurrence of inflammation, or, I should say, probably by the causes which gave rise to this condition: for it is of importance to observe, that the patient had, in my hearing, been frequently accused by the manager, (Mr Bembury,) of excess in drinking; and I have no doubt whatever of the fact, the manager being a very accurate observer of the habits of all under his charge, consisting of above 400 negroes.

The cure of *mal d'estomac* may, therefore, in this case, be referred to the use of stimulants taken to the extent of rousing the dormant powers, and producing such a degree of excitement in the system as to require active means to reduce the phlogistic state of the blood-vessels.

I consider this case an instructive one, and beg leave to submit the same to the consideration of the faculty.

Stafford Place, Pimlico.

ART. V.—*Pathology of the Eye, No. III.—On some of the Changes produced in the Coats of the Eye by Inflammation.* By ALEXANDER WATSON, Esq. Fellow of the Royal College of Surgeons, &c.

DISSECTION has not hitherto elucidated the organic changes produced by diseases of the eye, to the same extent that it has done those of the other parts of the body. In proof of this, I need only say, that very few dissections are detailed by the most distinguished authors who have written on these diseases; and that a general feeling of regret is expressed by them, that the diseases of the internal parts of the eye have not yet in this way been more extensively examined. The cause of so few dissections relating to this subject having been recorded is obvious. Very few of the diseases of the eye cause death; so that it is only by the accidental occurrence of some fatal malady in a person affected with a diseased eye, that an opportunity for its examination is afforded.

Inflammation and the organic changes produced by it are, in the generality of cases, confined to one particular part of the eye. But as its different parts are so delicate and so intimately connected to one another, the inflammation very often affects several or the whole of its coats at once. This remark is amply illustrated in most cases of severe ophthalmia. So that an eye which has been severely affected exhibits organic changes in several of its different parts.

The following are some of the organic changes which I have observed in the different coats of the eye from inflammation.

Conjunctiva.—The conversion of the conjunctiva covering the sclerotica and eyelids into a red fleshy thickened substance, having in some cases a copious infiltration of serous fluid into it, is a very common occurrence where it is violently inflamed. When this takes place in cases of acute inflammation, it is seldom permanent,—in time subsiding under proper treatment.

When the thickened fleshy state of this membrane takes place in a chronic form, it is generally only partial, as in the affection called pterygium and other morbid growths.

Cornea.—When the cornea propria is the seat of violent inflammation, it becomes highly vascular and thickened by the interstitial deposition of lymph producing the opaque specks of the cornea so often seen. Effusion of lymph or pus into its interstices, are sometimes followed by ulceration and sloughing of the cornea and its conjuctival covering. But a more gradual deposition of lymph into the substance of the cornea, (whether ulceration has taken place or not,) by becoming organized, occasions opacity and increased thickness and size of the part. This gradually projects forwards and produces the familiar disease of partial or complete staphyloma of the cornea. Fig. 1. Shows the great thickness which the cornea sometimes attains in this affection. All the other parts of the eye are nearly in their natural state, with the exception of the iris, which adhered to the inner surface of the cornea. The conjunctiva, though extended over the thickened and enlarged cornea, is nearly in its natural state.

Sclerotica.—Two changes of structure of an opposite nature take place in the sclerotic coat from inflammation. The one consisting of an increased thickening of the coat; the other of a diminution of its natural thickness or thinning of it. The *former* of these takes place in those cases where this coat is alone the seat of inflammation, or partakes of the inflammation affecting other parts of the eye. The *latter* appears to take place in cases where the other coats of the eye are chiefly affected.

Increased thickness of the sclerotica from interstitial deposition is probably of common occurrence in aggravated cases of ophthalmia, though it has been rarely observed, owing to its not giving rise to any peculiar morbid phenomena, and opportunities for dissection rarely occurring. Fig. 2 exhibits this change after violent inflammation, which continued for several weeks, and affected the whole eyeball.

Diminished thickness or interstitial absorption of the sclerotica gives rise to the peculiar appearance known by the name of staphyloma of this coat. Staphyloma sclerotica consists of a part of this coat projecting beyond the rest, and having a bluish colour, which is dark in proportion to the thinness of the part. This blue colour arises from the choroid coat being seen through the semitransparent conjunctiva and sclerotica. In Fig. 3. this existed to a slight degree at each side, at which parts the sclerotic coat was only the thickness of writing paper.

It seems to me probable, that the thinning and yielding of the sclerotic coat takes place in consequence of increased pressure from morbid accumulation of fluids within the eyeball, upon an inflamed portion of this coat. It occurs most frequently near to the cornea, where the coat is thinnest, and where it is most apt to become inflamed, from its proximity to the iris and ciliary ligament. In most of the cases in which I have seen this affection, the eyes may be said to have been dropsical; and in none of them had an opening been previously formed in the cornea or other parts, by which any of the humours could have been evacuated. In some cases the cellular tissue of the hyaloid membrane containing the vitreous humour has become disorganized and absorbed, leaving the eyeball filled with limpid fluid, and the lens floating loose amongst it. In others, a morbid accumulation of fluid has taken place between the retina and choroid coat, as in the case from which Fig. 3. was taken.

Iris and *Choroidea.*—Important alterations produced in these parts by inflammation are of frequent occurrence, and give rise to several interesting phenomena. The dissections which I am about to detail confirm the opinion which I have elsewhere stated, that in cases of what has commonly been termed *iritis*, the iris is not inflamed alone; but that the choroid coat, and, not unfrequently, other parts of the eye are also affected. This is precisely what was to have been anticipated from the great vascularity of the choroid coat,—its intimate connection with the iris,—and the violence of the symptoms which take place when the iris is said to be inflamed.

The common changes of structure which take place in the iris from inflammation, consisting of adhesions to the capsule of the lens, contraction, irregularity of form, and immobility of the pupil, and deposition of organized lymph, must be familiar to every one. But there is another change which takes place in some of the most aggravated cases of iritis, and which is of more rare occurrence. The change to which I allude consists of a red fungous-looking growth from the surface of the iris, arising from the organization of lymph effused upon its surface, in cases where the cornea has been laid open either by a wound, ulceration, or sloughing of it. Cases of this kind are occasionally presenting themselves, and their real nature is not generally understood. The red fungous growth projecting from an inflamed eye presents so formidable an appearance as to have sometimes induced surgeons to entertain the expediency of removing the eye. In several of these cases in which I have been consulted, the violence of the symptoms and formidable appearance of the organ have completely subsided by soothing treatment. In one case which I had an opportunity of seeing, the patient having died from other causes, I had an opportunity of obtaining his eye, which is delineated in Fig. 2. The cornea had been cut open, the iris and the other coats of the eye became violently inflamed, and continued so to the period of the patient's death, which happened at the end of two or three weeks. The appearances presented in the eye are increased thickness and density of the sclerotic coat; thickening disorganization of the choroid coats, with intimate adhesion of it to the sclerotica, and copious deposition of lymph upon its inner surface. A similiar deposition took place upon both sides of the iris, which is also much thickened. This lymph became organized, and presented the red fungous appearance above described. The retina and humours of this eye had been evacuated some time before death. Most of these morbid changes are exhibited in the plate.

The following cases show other important consequences of inflammation of the iris and choroid coat, taken from the notes of them kept at the time they occurred.

A. B. æt. 20.—April 1825.—The right eye has for several weeks past been affected with acute inflammation of the iris. The pain of the eye and head have been, and still continue, uncommonly severe, accompanied with great intolerance of light and lacrymation. Symptomatic fever considerable. Vascularity of conjunctiva not great. Cornea and anterior chamber has a muddy appearance. Was lately in the Royal Infirmary for a venereal affection. Both eyes afterwards became inflamed, but the left is now almost well. Has been treated with active antiphlogistic means by Dr Alison, who concurred with me in thinking that the evacuation of the aqueous humour by a puncture of the cornea might afford relief. This was accordingly done, and warm fomentations continued with relief for a few days. The pain having returned and continued very severe, accompanied with *muscae volitantes*, and sparks of fire before her eyes, she was again blooded repeatedly. Opiates were also given without much benefit. The sclerotic coat began to project a little above the cornea in the form and colour of a staphyloma. In a few days this increased very much, the whole eyeball became enlarged, and presented a very alarming appearance. The pain was at this time so excessive that I was induced to make a small opening into the prominent part of the sclerotica. My incision penetrated the conjunctiva, sclerotica, and choroid coats. Upon this an amber-coloured serous fluid was evacuated, which must have been situated between the choroid coat and retina, as none of the humours flowed out, and the eye did not collapse. The patient complained of the pain of the operation, but was afterwards easier. The pain again returned, and became very severe in a few days, along with much distension of the eye, so that I again punctured the coats, and repeated it several times afterwards with considerable relief. She then went to the country. I saw her some time afterwards, and observed that the eye was in a collapsed state.

How far the opinion I had formed as to the nature of the above case, and the situation of the effusion, was correct, the reader will be able to judge from the following.

C. D. æt. 60.—Oct. 25, 1828.—The right eye is much inflamed. The iris has lost its proper colour; aqueous humour is muddy; ciliary and conjunctival vessels much enlarged; pain of eye very severe, darting through the head in different directions; pupil rather larger than the other; eye insensible to light. Complains also of distracting pain over the whole side of face; great lacrymation; pulse hard and strong.

Eye became inflamed about three weeks ago, when she experienced complete relief for two days from copious blood-letting. The inflammation again returned, and has continued ever since. Says that the sight of this eye had been going gradually away for three weeks previously to its becoming inflamed. Had a paralytic stroke upon the same side of her head eight years ago, which in a great measure gradually got well.—Fifteen ounces of blood taken from the arm, and warm fomentations applied.

Nov. 1. Felt greatly relieved by the bleeding and fomentations. She has got purgatives. Leeches were applied to eye, and side of head rubbed with anodyne liniment. Last night got some Dover's powders with antimony. To-day the inflammation of eye is greatly diminished. Iris adheres to the capsule of the lens at the nasal side by an effusion of lymph. Pupil is of a green colour.

9th. Eye still somewhat inflamed. Has complained much of pain over all the parts supplied by the fifth pair of nerves of the same side of face.—Same remedies continued, with the addition of quinine three times a-day.

13th. Is better one day and worse another. Eye is better, but there is still considerable sclerotic vascularity, particularly around the cornea. Sclerotica projects a little at the upper part near the cornea, and has got a bluish leaden colour, forming a staphyloma of this coat. Pain of the nerves of the face is often very severe. Pulse good.

Dec. 8. Opium wine has been dropt into the eye with benefit. Pain has continued better and worse. Some days free from it; other days very much distressed with it. Began to take mercurial pills about ten days ago, till the system was affected, when the pain became decidedly better, so that the pain and inflammation of eye are now greatly lessened.

Feb. 1830. Continued gradually to recover, but without any vision being restored. Eye has since continued free from pain and inflammation. The cornea is transparent; pupil dilated, irregular, and adhering to lens at one side. Retina seems opaque, and the humours appear turbid. Sclerotica staphylomatous to a slight degree.

Sept. 21. Died in an apoplectic state after two or three hours illness.

Inspectio Cadaveris.—Ventricles of brain contained from six to eight ounces of grumous blood with extensive softening and disorganization of the brain. Arteries of the brain had an enlarged, thickened, and speckled appearance, from morbid depositions between their coats, with also a few ossified points. Aorta in the same state.

Right eye larger than natural; of an oval form anteriorly, from the bulging of the sclerotica, which had a bluish appearance where it projected. When cut open, sclerotic coat remarkably thin at the projecting parts. Choroid coat nearly transparent from the absence of pigmentum nigrum. Amber-coloured fluid flowed out from the interior of choroid coat. This fluid contained a considerable quantity of concrete cholesterine in small scaly particles. This fluid occupied about two-thirds of the posterior part of the eyeball; the compressed retina and humours occupied the remaining third anteriorly. The posterior part of the retina is compressed like a cord, while its anterior part extends from about the middle of the eye to the ciliary zone where the retina terminates. The retina is seen extended over the compressed remains of the vitreous humour. See Figure 3. The optic nerve was rather smaller than natural, and had a semitransparent cartilaginous appearance.

These two cases exhibit instances of acute inflammation of the iris and choroid coat terminating in effusion into the retino-choroid cavity. The displacement of the retina and the compression of it and the ciliary nerves, by the distension of the eyes, account sufficiently for the loss of sight, pain, and palsied state of the iris that took place. The pain diminished as the vitreous humour became absorbed, by lessening the pressure upon the ciliary nerves. And it is probable that the painful affection of the branches of the fifth pair of nerves arose from sympathy with that of the ciliary nerves. In the second case, however, as vision was said to have been lost before the eye became inflamed, and as that side of the face had been affected with paralysis some time previously, from which it had never altogether recovered, and as there was also a much diseased state of the arteries, which must have existed for some length of time previously to death, it is impossible to say how far the disease of the eye and side of the head depended on the state of the brain, or what particular connection they had with each other. I have had occasion to see several other cases of violent disorganizing ophthalmia following affection of the fifth pair of nerves, but have not yet ascertained their particular connection. In one case of this kind Dr Alison found on dissection that the fifth pair of nerves at one part was much contracted and almost completely severed.

The above cases show that what is called inflammation of the iris is not confined to the iris alone, but that the coats, particularly the choroid, also partake in the disease. They also show how little relief could have been obtained from the evacuation of the aqueous humour, and how much by puncturing the sclerotic and choroid coats by means of a grooved needle, to evacuate the effused fluid.

In the above cases, if the inflammatory effusion contained within the choroid coat had become absorbed, the coats of the eye would have gradually shrivelled and collapsed. This, therefore, is probably the pathological state of those eyes where disorganizing inflammation is followed by a gradual atrophy of the globe. This took place in the case A. B. and in several other young and middle-aged subjects that I have seen. The above idea as to the pathology of this state is in a great degree confirmed by a preparation of a collapsed eye in my possession. In this eye the retina presents the appearances above described and delineated in Fig. 3.

The retina and humours of the collapsed eye have obviously been compressed in a similar manner from fluid contained within the choroid coat. See Fig. 4, in which this eye is represented. With the history of this case I am unacquainted, but I could discover no mark of any opening having existed in any part of the coats of the eye by which the humours could have escaped.

Retina and Optic Nerve.—The morbid anatomy of the retina is of very difficult investigation. This arises from several causes. *1st*, The great tenuity and delicacy of its natural structure render it probable, that changes not cognizable to our senses may have existed and occasioned blindness. *2d*, The retina, though naturally transparent during life, always becomes opaque soon after death, so that no correct inference can be drawn from its appearance in this respect, notwithstanding it is probably one of the highest importance.

The changes of structure which the retina undergoes in disease are as yet very little known. The preparations in my possession illustrate two changes. In the preparation from which Fig. 3 was taken, the retina is not only condensed, but appears to be much thickened in its texture; and the optic nerve, in place of having its opaque medullary appearance, was semitransparent like soft cartilage, and diminished in size. The optic nerve of the collapsed eye represented in Fig. 4, is likewise so much diminished in size, that very little remains except its sheath. Was this owing to the retina having become insensible and useless?

Another eye which had been much inflamed from a wound with a fork, shows one-half of the retina detached from the choroid coat thickened, and somewhat corrugated.

The other change in the retina above alluded to, consists of a thinning of its texture, apparently from the want of medullary matter. This is seen in the eye of an old man, who had lost the sight of it from staphyloma of the cornea by ophthalmia during small pox. Whether this want of medullary matter had been caused by some change in the organization of the retina, in consequence of the previous inflammation, or arose from the want of exertion of the organ, it is not easy to determine. He could, however, distinguish light from darkness with it. The *foramen centrale* and fold of Sœmmering seemed both wanting in this eye.

In the eye of an old woman whose vision had been very imperfect before death, the retina presents the same attenuated appearance, from deficiency of medullary matter.

EXPLANATION OF PLATE.

The figures are all taken from preparations in my possession.

Figure 1. Eye of a girl affected with staphyloma of the cornea.

a. The thickened cornea covered by the conjunctiva.

b. The conjunctiva upon the sclerotic coat.

c. The coats of the eye, with the crystalline lens *in situ.*

Figure 2. Eye from a middle-aged man, which had been affected with violent inflammation for several weeks. The eye has been cut open, and the posterior part raised by which it is suspended.

a. a. The thickened sclerotica.

b. b. The thickened choroidea adhering to the sclerotic coat.

c. Masses of lymph effused in the inner surface of the choroid coat.

Figure 3. Eye described in case C. D.

a. a. The sclerotic and choroid coats, within which fluid was contained.

b. c. The optic nerve and retina. The latter is seen compressed posteriorly, but extended over the remains of the vitreous humour anteriorly.

Figure 4. Collapsed eye, in which the coats have the same relative position towards each other as in Figure 3.

a. The cornea.

b. The condensed remains of the vitreous humour covered posteriorly by the retina.

c. c. The sclerotic and choroid coats which contained fluid.

d. The flaccid sheath of the optic nerve, in which the latter was nearly altogether wanting. The condensed retina is seen passing from the posterior part of the coats to the remains of the vitreous humour.

The eye had been in this state for several years.

Art. VI.—*On the follicular Origin of some Vaginal Tumours.* By George Oakley Heming, Esq. Member of the Royal College of Surgeons London.

Sir Astley Cooper has, in a very interesting paper, shown that some encysted tumours consist in enlargement of cutaneous follicles; and in the course of his work upon hernia, that gentleman has described a similar tumour originating in enlargement of a mucous follicle, situated just below the *meatus urinarius* in women.

It has not, I believe, been hitherto conjectured that some of those tumours which are known occasionally to occupy the pelvis and obstruct parturition have a similar origin. This fact appears, however, to be distinctly established by cases which have fallen under my observation; and it is the more important, because it immediately suggests the propriety and safety of the treatment by free incision.

I have carefully examined the bodies of two women in whom I found tumours of this description projecting into the vagina; in one there were two of these tumours, in the other there was a single one as large as an egg. On a minute examination of their internal structure, it was evident that they consisted of obstructed lacunæ, which had thereby become dilated into a cyst, and distended by a gelatinous fluid. I was enabled to trace distinctly in the smallest tumour a continuation of the mucous membrane of the vagina into the tumour, and a reflection of this membrane forming the lining to the latter. I can have no doubt that the tumour in Mrs Hollingsworth, the particulars of which I am about to detail, was of the same nature. Mr Vincent as well as myself was convinced of this fact; and it is probable that the greater number of those tumours which obstruct parturition, and which have been described by the authors who have written on this subject, were of similar origin. If this be the case, I think no one would doubt that when they existed in labour, so as to obstruct the descent of the child, the best practice is to evacuate, and thereby diminish them by a *very free opening*.

This view of the subject is further confirmed, if that were necessary, by the history of the cases of this kind which are recorded. Perfect, Denman, Park, Merriman, Davis, and Drew, have each described cases in which tumours were found between the vagina and the rectum at the commencement of labour, which, from their bulk, afforded greater or less impediment to the passage of the child. Some of these tumours were proved by examination after death to have been diseased ovaries; others were concluded to have been ovaries, although sufficient proof of this fact was wanting. But in others the histories of the cases show that they could not have been ovaries; but they leave the nature of the tumour in complete obscurity.

Some which were not opened disappeared spontaneously after delivery, leaving the practitioner to conjecture what they could have been. Others which were opened through the vagina or through the rectum, discharged a bloody serum with membranous flakes, and became thereby collapsed; others, during an attempt to lift them above the brim of the pelvis, disappeared with a sensation of bursting; and one, an account of which is given by Dr Drew in the first volume of the Edinburgh Medical and Surgical Journal, was extirpated by an incision in the perinæum. In this doubtful state of our knowledge concerning the nature of tumours which are not of unfrequent occurrence, which, when they do occur, occasion so material an impediment in the process of parturition, and about the nature and treatment of which the minds of practitioners are so very unsettled, it is important to establish the fact of the follicular origin, and safe treatment of some of them by incision.

Besides the proof of the first of these facts already given from dissections, I am enabled to add that of the second by a case which fell under my care some time ago.

Mrs Hollingsworth came to me in April 1822, with a tumour in the vagina, which a surgeon whom she had previously consulted told her was *prolapsus uteri*. I found an oval tumour situate between the *vagina* and the *rectum*, its attachments to either of these parts were so loose, that I could, by putting my finger beyond it, hook nearly the whole of it out of the vagina. It could not be *prolapsus*, for the neck of the uterus could be felt above it in its natural situation; and the same circumstance, together with the absence of the symptoms of pregnancy, proved that it could not be retroversion of the uterus. As the tumour, from its situation and bulk, was very inconvenient, the patient wished to have it removed; but before doing it, I advised her to consult Mr Vincent, who agreed with me in thinking that this might be done with safety. I therefore proceeded to perform the operation. On cutting into the tumour, I found that it consisted of a cyst containing a considerable quantity of glairy fluid. This was evacuated, the cyst was left in its situation, and the patient was well in a few days. Three months elapsed, at the end of which time the patient came to me again, stating that the tumour had returned; that it was considerably larger than the first time she applied to me; and that she wished I could remove it entirely. This I did by simply dissecting it out. The operation was attended with very considerable hemorrhage, which, however, was stopped by plugging the vagina with lint, and in three weeks she was quite well.

The great point is the diagnosis. This may be distinctly established by carefully tracing the origin of the tumour. The conduct of the practitioner may then be both prompt and confident. A free incison at the period of parturition, and excision at any other time, will safely relieve or cure the patient.

Art. VII.—*Operation for restoring the Columna Nasi.* By Robert Liston, F. R. C. Surgeons, one of the Surgeons to the Royal Infirmary, Lecturer on Surgery, &c.

Case I.—More than seven years ago I was consulted by Mr M., then aged 16, on account of deformity occasioned by loss of the *columna nasi*, of the cartilaginous septum, and of part of the osseous septum, in consequence of external injury. At that time I proposed, as soon as the discharge had ceased, to furnish him with a new columna from the upper lip, and had several conversations with him and his friends on the subject. The proposal, however, was not acceded to, and I lost sight of Mr M. till the beginning of 1828. In July 1828 I performed the operation as originally planned. The patient's head being held backwards, the under surface of the point of the nose was pared, so as to present a raw and concave surface; a bistoury was twice passed through the upper lip, close to the root of the original columna, and each time carried forwards to the mouth in a straight direction, and with little sawing motion, so as to include a slip about a quarter of an inch in breadth. This slip, composed of skin, mucous membrane, and the interposed tissues, was then deprived of its prolabium, and elevated without twisting, so that its oral margin was placed in contact with the raw surface on the tip of the nose; and in this position it was retained by a point of convoluted suture, a pin being passed obliquely through the point of the nose and the upper part of the new columna. The raw edges in the wound of the lip were brought into accurate apposition by two points of twisted suture, as in the operation for harelip.

The pins and ligatures were removed after a few days, and adhesion was found to be completed. The lip, which before was too full and dependent in the centre, had united with very little mark, and was materially improved in appearance. The union of the upper portion of the slip was also perfect; and by supporting this part by a small round compress, and carefully plugging the nostrils, so as to distend the alæ, the patient's appearance was totally changed. The point of the nose could not fall downwards; the alæ were not shrunk and approximated to each other, but tense and natural; and the cavity of the nostrils was not exposed, presenting the appearance of a dark and foul sore, but hid and protected by the firm and fleshy new columna. The patient, instead of being constrained by horrid deformity to confine himself to his house and surrounding grounds, was able to mix in and enjoy society, without its being observed that any operation had been performed to improve his countenance. That part of the membrane of the mouth forming the outer surface of the new columna, remained reddish for some time, but by exposure gradually assumed the same colour, and apparently the same structure, as the surrounding skin.

Case II.—Anna Riley was admitted into the Royal Infirmary on the 10th of August 1828. There was very copious and fœtid discharge from the nostrils; the triangular cartilage and columna nasi were completely destroyed; and the inner surfaces of the alæ extensively ulcerated. The point of the nose had become quite flat and depressed, from the loss of its natural support. The disease was of six months' duration, and commenced without evident cause.

On the 7th of October, ulceration had ceased, and I formed a new columna, in the same manner as has been already described. The parts united by the first intention, and the operation succeeded perfectly.

On the 27th she was dismissed with her features greatly improved.

Case III.—Mary Anne Love, aged 11, was admitted about eighteen months ago, labouring under *lupus.* The alæ of the nose, the upper part of the lip, and the inner surface of the nostrils, presented one continuous surface of angry ulceration. The *columna nasi* and part of the cartilaginous septum were destroyed, and the point of the nose was flattened and depressed. The discharge from the ulcer was acrid and highly offensive, and the countenance was very much disfigured.

The disease had existed for six months previously to her admission; and during that time various applications had been employed with the view of checking the ulceration, but without effect.

In the Infirmary means were taken to improve her general health, and the sore was touched occasionally with spirit of turpentine. Under this application the ulceration seemed to be arrested for some time, and the aspect of the sore began to improve; but the benefit was temporary, and the liniment, having lost its influence over the irritable surface, was disused. A solution of the nitrate of silver was then employed, and that also, though at first beneficial, gradually became inefficacious. Solutions of the sulphates of zinc and of copper were afterwards had recourse to; and by changing the above applications, according as each became inactive, the sore was brought into a healthy state, and the process of reparation commenced. The topical remedy which all along proved of most service, and under the use of which the parts were ultimately brought to cicatrize, was the spirit of turpentine.

By the middle of last May, cicatrization was almost complete, and I prevailed on the patient to have her deformity removed by the formation of a new columna. The operation was performed in the same manner as in the preceding cases, and adhesion was completed in both the nose and lip in two or three days. The columna was supported by compress and bandage, and the alæ were kept distended by dossils of lint.

Ulceration has not returned; and the margins of the alæ, which were not quite healed previously to the operation, are covered with thin crusts, and apparently cicatrizing. The change in her appearance is very flattering, and promises to be still more so when œdema leaves the part.

Case IV.—In the summer of 1827, I performed the Indian operation for restoration of the nose on Charles Thorne, and gave some account of his case in the 92d number of this Journal. I then stated that the operation had completely succeeded, except in the columnar part, and that I intended to repair that deficiency as soon as the patient would submit to farther procedure. To this, however, he was averse, and left this part of the country.

In August last he again presented himself, and was now anxious that the operation should be performed, as the point of the nose had necessarily fallen much down from want of mesial support. I made him a new columna (the third he had had) from the upper lip, having previously elevated the point of the nose as much as possible. The parts adhered quickly and firmly, and he left the Infirmary much pleased with the support and improved appearance which the *new feature* of his countenance had obtained.

Case V.—Mr R. H. enjoyed good health till April 1827, when he had a smart attack of tertian ague, which yielded to the use of sulphate of quinine. In the following August, after severe mental exertion, he complained of pain in the head and general indisposition. He was advised to abandon professional pursuits for a season, and went to Brighton, where he was seized with violent pleurisy. Early in October he had another attack of tertian ague, with severe pain in the right side of the head.

Aguish symptoms continued to harass him till December, after which he remained well till April 1828, when he was again affected with ague, pain of the head, debility, &c. and these symptoms returned in January following.

In July 1829, he was sent to Leamington, and while there encrustations began to form in the nostrils, and on the separation of the crusts fœtid discharge occurred. The discharge continued till the latter end of September, when he went to London. There his case was pronounced to be one of secondary syphilis, though the patient declared then, and declares still, that he never had primary symptoms; and, accordingly, he was ordered blue pill, sarsaparilla, and eventually mercurial friction. His mouth soon became affected, and the salivation was very profuse. In October the bones of the nose and palate began to exfoliate, and the patient became much reduced. Early in November severe inflammation of the eye and of the side of the face supervened; and Mr H. was bled, purged, and starved. The inflammatory action was soon subdued, but was followed by a violent attack of diarrhœa, which brought the patient very low. From this time he regained strength gradually, and now he enjoys excellent health.

He applied to me in August last. The exfoliation has been extensive, and caused apparently by the abuse of mercury. In the posterior part of the palate there is a large deficiency, which the patient is obliged to supply by a metallic substitute; and the lower part of the osseous septum is destroyed.

The cartilaginous *septum* and *columna nasi* were gone, and the nose lay quit flat on the face, with its wrinkled alæ sunk on the floor of the nostrils, and its point adhering to the upper lip, where the root of the columna had formerly been.

The first thing to be done in this case was to prepare the parts for the columnar operation; and, accordingly, I divided the attachment between the point of the nose and the lip, removed the ruinous remains of the columna, and separated some adhesions within the nostril that had formed during the cicatrization, raised the apex of the nose, and distended carefully its alæ. By these means even, the appearance of the patient was much altered, and he began to be satisfied with what had been done. He was persuaded, however, to get a more durable and elegant support for the parts than dossils of lint, and underwent the columnar operation on the 31st of August.

In this case, as in the others, union took place by the first intention; and, I need scarcely add, that the result is very satisfactory.

Such is a short outline of those cases in which I have had occasion to form a new *columna*. The deformity caused by the loss of this prominent part is very great, indeed almost equal to that occasioned by destruction of the whole nose; and an operation for the removal of such deformity, so simple, so effectual, and, I may add, so neat, cannot fail to be approved. The lateral slip, destined to form the new columna, should not be twisted round so as to present its cutaneous surface externally, but merely elevated and affixed to the point of the nose. By twisting, the chance of success is diminished, and if the part does adhere it is thick and clumsy. The mucous membrane forming the inferior surface of the columna retains some of its characters for a few weeks, but gradually assumes a cuticular appearance. For some time after the operation, on tickling or compressing it, the sensation is referred to the inside of the mouth. Though it might be supposed that in a male adult, the beard on the inner surface of the new columna would prove a source of irritation, this is not the case; the hairs lose their stiff and bristly character, not being cropt frequently as before, and being constantly moistened by the mucous secretion of the nostrils. In fact, they come to resemble those hairs that grow naturally from the parts.

In the above cases the incisions always adhered by the first intention; and I should think that if proper care be taken to place and preserve the raw surfaces in accurate contact, adhesions will always occur, little or no mark remaining. Indeed, the appearance of the upper lip will always be benefited by the operation, for when the columna is lost, the lip becomes elongated and tumid at its centre; and by a narrow slip being removed from it this defect is obviated, whilst the cicatrix, from being in the situation of the natural depression, is scarcely observable.

In the after-treatment it is necessary to keep the alæ tense by dossils of lint, in order to assist the columna, not yet sufficiently consolidated, in supporting the parts in their natural situation; and when the columna itself becomes tumid, a compress and bandage should be neatly applied over it.

When the cartilaginous septum is destroyed, as is generally the case, of course an aperture remains between the new columna and the osseous septum, but that is not perceived unless on close examination, and does not annoy the patient.

In a recent number of the Journal Hebdomadaire, a case is recorded, in which attempts had been made to restore a lost columna, first by M. Dupuytren of Paris, and afterwards by M. Gensoul of Lyons. The operation failed, and the nose became approximated to the lip. M. Dupuytren raised a *flap of integument* from the lip, and adapted it to the nose, after having *twisted it round.* Had he formed his columna from the *whole thickness of the lip*, there would have been no necessity for *twisting* it, and in all probability the operation would have proved successful.

N. B.—The two first cases are detailed in the London Medical Gazette for April 17, 1830.

99, George Street,
September 10, 1830.

Art. VIII.—*Observations on the influence of imperfect supplies of fresh Air, long continued, on the general Health.* By William Watson, Esq. Surgeon, Wanlockhead.

From the experiments of Lavoisier, Davy, Allen and Pepys, and Dr W. F. Edwards, we learn, that air containing a certain proportion of oxygen gas is indispensible to effect the requisite changes on the blood of the pulmonary artery; that in the course of successive respirations, the oxygen gradually diminishes and that its place is supplied by variable proportions of carbonic acid gas; and that, in consequence of these changes in the air respired, frequent fresh supplies are requisite. It was also ascertained by Allen and Pepys, that when an animal breathes the same quantity of air without change, death ensues before all the oxygen disappears; and that when the same portion of air is respired until it can no longer effect the requisite changes, it still contains only 10 per cent. of carbonic acid.

In all the experiments of these observers, the animals were made to respire an atmosphere which was gradually becoming vitiated, until insensibility, or fatal *asphyxia* took place; and no attention was paid to the question, how far an atmosphere not frequently changed is capable of supporting life, and what influence such an atmosphere exercises on the functions and health of the animal body?

This question, I think, may be illustrated, if not determined, by the situation of miners, who are under the necessity of following their avocations in places so remote from the general mass of the atmosphere as to be almost insulated; and where, of course, there is neither a regular ingress of atmospheric air, nor egress of the substances formed during respiration and combustion. In these places the air is so rapidly inhaled that a considerable diminution of the quantity of oxygen must follow, and its place being supplied by an equal quantity of carbonic acid gas, the bulk remains unaltered. The carbonic acid, however, from its specific gravity falls to the bottom of the mine, where its presence is indicated by a candle burning more obscurely than when placed at a greater height.

But carbonic acid is scarcely ever accumulated in such quantities as to affect the action of breathing, except in so far as that is connected with the process of oxidation and assimilation; and then its effects are very material; since all the nutritive particles, before they can be assimilated with the blood, must be submitted to the action of the atmospheric air through the medium of the lungs. " In this way," to use the words of one of our ablest philosophers, " respiration may be said to be carried into every part of the body." Since, then, a certain proportion of oxygen is so necessary for the due support of the animal system, and since it has been demonstrated by satisfactory experiments, that a medium, consisting entirely of oxygen, would excite the action of all the vital functions to such a degree as even to cause the extinction of life, it seems a natural inference, that the inhalation of a medium deprived of a part of its oxygen should be attended with very different results—namely, a loss of tone or debility in proportion to the quantity inhaled.

That this is really the case, is not only proved by the experiments above alluded to; but all the observations which I have made during a considerable number of years tend to strengthen my conviction of its truth.

In the year 1823, sixteen men were employed, during four months, in a mine where the deficiency of air was so considerable, that a candle would not burn for any length of time, except when the wick was made so open as to allow the greatest possible quantity of air to come in contact with the ignited part. The candle was placed generally in a sloping position. Notwithstanding the deficiency of air, none of the miners had any inflammatory complaints, but all complained of lassitude, debility, and drowsiness, particularly towards the end of each day's work, sixteen hours; and they all became gradually paler in the complexion.

In 1824, an equal number of men were employed in the same mine with nearly the same result. Thirteen were quite free from any particular complaint,—one complained for a few weeks of a slight pain in the stomach,—the other two for a few days of pain in different parts of the body from obstructed perspiration.

In 1826, sixteen men were employed five months in a mine where the quantity of air was still smaller than in the former, still the miners were free from any particular complaint. Only one complained of shifting pain from improper exposure when perspiring freely. In 1827, the same number of men were employed in the same mine for four months, and all remained free from complaint. Reasoning, then, from analogy, it seems fair to conclude, that a person placed in a medium where there is a considerable deficiency of oxygen must become less liable to inflammatory action, and that this non-liability will be in direct ratio to the length of time he is confined in such medium. At least, this is no speculation founded on mere conjecture, but supported, in my opinion, by facts which have come under my own observation. From these, I draw the following inferences:—1*st*, That miners are not more liable to inflammatory complaints than any other class of the community. 2*dly*, That a majority of them, at least in this district, remain free from any particular affection of the chest. 3*dly*, That hydrothorax among miners is rather a rare disease, only two cases having occurred to me in the course of fifteen years' practice.

With regard to the first of these conclusions, I may merely mention, that a majority of the inflammatory affections of the chest which have come under my notice, arose from exposure to sudden vicissitudes of weather, as in the cases of engine-keepers, lead-washers, carters, &c. and as to the second,—of sixteen miners, each sixty years of age or upwards, and still connected with the works, four have been invalided; while the other twelve continue to follow their occupations as usual. The united ages of the miners invalided amount to 329.

I may also observe, that from a professional observation of fifteen years in this mining village, I have been led to the conclusion, that phthisis is not more prevalent among the miners here, than among any other description of people who are constantly inhaling pure atmospheric air.

As a proof of this, during the above-mentioned period, cases have occurred as frequently among females as among males; and even of the latter, there were several individuals who had never been employed in the mines at all, so that, with reference to this disease, the balance seems to be in favour of mining as an occupation; the deaths having been, according to my computation, in the proportion of six miners to nine others. I infer from this also that the effect generally attributed to the inhalation of noxious vapour and fine mechanical particles, namely, the irritation of the mucous membrane of the bronchia, and the inflammation thereby induced, has been much overrated; and that the conclusions drawn from this supposed effect are founded rather on analogy than on experiment or observation. The idea, that fine mechanical particles may be suspended in the atmosphere, and, being inhaled along with that fluid, may produce the irritation alleged, I am inclined to receive with considerable distrust. Mechanical particles, to occasion any serious injury, must be of such a magnitude as would preclude the possibility of their being retained in suspension; and, on the other hand, such as from their extreme minuteness would admit of being so suspended, may either be inhaled with impunity, or must do their mischief in some other mode than the one assigned. It appears to me, therefore, that, if consumption does occur more frequently among miners than among some other classes of the community (a position which I think questionable) it must arise chiefly from the laborious exercise necessary in their daily avocations, which, by accelerating the current of the blood through the lungs, may occasion some injury to that delicate organ, especially if any part of the vascular system was predisposed to disease. But to this I would consider them no more liable than any description of labourers on the surface whose stated exercise is equally severe. I am aware that this view of the subject differs from that of many medical gentlemen of established name; yet it has not been hastily or carelessly adopted, but is founded on long observation and experience; and, unless it can be invalidated by opposite experience, I flatter myself that the liberal and enlightened practitioner will hesitate to pronounce it altogether erroneous.

Wanlockhead, 19*th June* 1830.

ART. IX.—*Case of Excision of the Elbow-Joint.* By JAMES SIMSON, M. D. Fellow of the Royal College of Surgeons Edinburgh, and Surgeon to the New Town Dispensary, &c.

IF we are to judge of the opinions of the French and English surgeons as to excision of the elbow-joint; from a paper by M. Roux in the January number of the Revue Medicale; or from Mr Lawrence's recent lectures, we must be convinced that it is not a popular operation either in Paris or London; and in Edinburgh there are surgeons of considerable reputation who maintain that this operation is only applicable to young persons, and never to be advised in cases where the individuals are advanced in life. I hope, therefore, that the following case may be of use in showing, that a tolerable joint has been made, and a very useful arm and hand preserved to a woman pretty far advanced in years, and where circumstances occurred not the most favourable to the completion of the cure. The manner in which the operation was performed is different from the method generally recommended, but is one in which, if the soft parts are in a proper state, it can be easily accomplished.

Mrs Rodgers, æt. 46, November 20, 1829.—The left elbow-joint is increased in size, and the integuments have a soft and puffy feel. There are four distinct openings discharging matter, through each of which the bone can be felt bare. Two of these openings are situate over the inner condyle of the humerus, the third over the external, and the fourth over the anterior part of the joint, beside the tendon of the *biceps* muscle. The motion of the joint is exceedingly limited, and she cannot move it, or attempt to shut the hand without great pain. Her health has fallen much off within the last six weeks, and she was troubled very recently with a severe bowel complaint. This complaint began in March, with severe shooting pain in the region of the joint, generally increased during the night, and always aggravated by motion. After the pain had continued for some time, swelling took place, and afterwards the sinuses formed, and have continued to discharge matter constantly for nearly two months. Various remedies have been tried, but without any relief.

As her general health had always been good till the beginning of this complaint; as the soft parts were not much diseased; and as the caries appeared confined to the extremities of the bones, I thought it a fair case to try excision of the elbow-joint, and was happy to find that Sir George Ballingall had the same opinion.

On the 26th November, the operation was performed in the following manner. An incision was begun on the back of the arm, about three inches above the joint, and carried in a straight line to about two inches below it, over the point of the olecranon process to the edge of the ulna, taking care to avoid the ulnar nerve. At each extremity of this incision a transverse one was made about two inches long, and the flap on the radial side was disseeted back, exposing part of the *humerus*, *radius*, and *ulna*. The flap on the inside was next made, taking care not to cut the ulnar nerve. The olecranon process was found carious at its upper part, and slightly adhering to the humerus. It was easily removed by the cutting pliers, as also a small piece on the back of the ulna that did not appear sound. The insertion of the *brachialis* muscle was not disturbed. The humerus was much diseased on its radial side, both in the joint and shaft of the bone. As about one-fourth of the articulation of the humerus on the ulnar side was sound, an oblique incision was made by the saw, so as to include three-fourths of the articulation, and extend about two inches up the shaft of the bone, and include all the diseased parts. The cartilage was removed from the sound part of the humerus, as also from the top of the radius, which was not in the least diseased. No artery required to be tied. The parts were secured by stitches, and the arm put in a slightly bent position. The greater part of the wound healed by the first intention, and in three weeks she could move her fingers without any pain, which she had not done for nine months before. By the end of December the wound was entirely whole, and she was walking about, and moving slightly the joint, without any pain.

At this time, however, her general health began to suffer from repeated attacks of bowel complaint, and several abscesses formed in different parts of the cellular substance of the fore-arm, which prevented her from moving either the hand or the elbow-joint without considerable pain. These abscesses continued to trouble her constantly for nearly three months, notwithstanding a variety of constitutional treatment; but at the end of March her health began to improve, and she has continued free from any complaint since that time. Her arm remains now in a slightly bent position, and cannot be put in a straight line with the shoulder; but she can bring her hand to her mouth, and is in the daily habit of using the arm in all her domestic duties, and can carry with it a large iron fender without any pain. She says she is very sensible that the strength of the joint has increased within the last three months, and that it is still improving. The connection of the radius with the humerus does not appear so firm as that of the ulna.

To Sir George Ballingall, I feel myself under many obligations, not merely for his attention in this case, but for his valuable advice and assistance in a great many others.

42, Frederick Street, 30th October 1830.

Art. X.—*An Inquiry on some disputed points in the chemical Physiology of the Blood and Respiration.* I. *On the Mutual Action of Blood and Atmospheric Air.* By Robert Christison, M. D. F. R. S. E. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. [Read before the Royal Society of Edinburgh, March 1830.]

THE science of Physiology ought in strictness to consist in the assemblage and generalization of facts. Such is the character which it is at length gradually assuming, now that the theoretical speculations which amused our predecessors have lost every attraction except what belongs to them as incidents in the History of Medicine. But it is a prevailing complaint, that the component facts of this science are vague and uncertain in a lamentable degree, owing to the contradictory statements which have been made even by acknowledged authorities. That this is not an unfounded accusation will sufficiently appear from the sentiments lately expressed by *M. Magendie* and *Dr Bostock*, the latest systematic writers in their respective countries; who confess and complain, that Physiology is still with many a work of the imagination, and that every step of the student is on debateable ground.

In these circumstances, it will scarcely be doubted, that a material service would be rendered to physiological science by any one, who would undertake to examine the researches of previous inquirers where they differ, with the view of settling, by a patient and impartial experimental comparison, where the truth lies between the contradictory results at which they have severally arrived. And investigations of the kind appear peculiarly called for in the present day; as chemistry, physiology, and pathology have supplied us with much more accurate instruments of research than were known even to our immediate predecessors.

Such are the impressions under which I have commenced a series of experiments on various disputed points in the chemical physiology of the blood and respiration. There is no other topic in physiology as to which the followers of this science are less agreed, if we except the functions of the nervous system: And yet it appears to me that there is scarcely any other subject of uncertainty, as to which positive results ought to be so certainly obtained.

In laying before the Society an account of my progress, I can follow conveniently no other order but that which is dictated by my opportunities of investigation. But the whole inquiry will be wound up with a connected view of the facts which may appear to have been ascertained.

The subject of the observations in the present paper is the *Mutual Action of Blood and Atmospheric Air*. And the object of the inquiry is to ascertain what changes really take place, and whether the arterialization of the blood in the lungs is a vital or a physical process.

Physiologists and chemists have generally considered, that when venous blood is brought in contact with atmospheric air out of the body, the blood changes its colour from dark purple to bright crimson, while the air loses a part of its oxygen and acquires carbonic acid. *Lower* in 1669 * appears to have been the first who noticed the change which takes place in the colour of venous blood; *Priestley* † first observed that oxygen is absorbed from the air; and *Girtanner* ‡ was among the first to discover that carbonic acid gas is formed. These experiments have often been repeated since, and commonly with the same results. They were repeated in particular on a large scale in 1809 by *M. Berthollet*,* who found that when a quantity of fresh blood was left twenty-four hours in 1768 cubic inches of air, oxygen disappeared to the extent of 68½ cubic inches, while exactly the same quantity of carbonic acid was formed.

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* Tractatus de Corde, p. 178.
† On Air, iii. 75.
‡ Appendix to *Beddoes* on Calculus, p. 219.

As the changes now mentioned in the blood and the air form the leading particulars in the phenomena of the arterialization of the blood in its passage through the lungs in the living body, it has been a generally received doctrine in physiology, that the arterialization of the blood,—so far as concerns its change of colour and effect on the inhaled air,—is essentially a physical, and not a vital process.

But very lately the accuracy of the facts, on which this doctrine is founded, has been called in question by *Dr John Davy*. *Dr Davy* has stated as the result of many trials, that atmospheric air and blood recently drawn from a vein have no mutual action whatever;—that the colour of the blood is not changed, that no oxygen disappears from the air, and that no carbonic acid is formed in it. He adds, in explanation of the discrepancy between himself and all previous experimentalists, that they left the blood in contact with the air for twenty-four hours or upwards after it was drawn from the body; that the blood, however, begins to decay in so short a period as twelve hours; that consequently the changes they remarked in the air were the effect of incipient putrefaction of the blood; and that if the experiment be made within twelve hours after the blood is drawn, no change whatever takes place either in the blood or in the air.†

These statements, proceeding from so high a chemical authority as *Dr Davy*, are deserving of serious attention, as they are sufficient to throw great doubts over the prevailing opinion, that the arterialization of the blood is a physical and not a vital process. I have, therefore, made many experiments on the subject with all possible care, some of them, indeed, before *Dr Davy's* paper was read, but most of them since; and the succeeding narrative contains the results.

I shall first consider the question as to the change the blood undergoes in colour by being placed in contact with air, and then the question as to the change which the air undergoes in its turn

I. The effect of agitation with air in changing the colour of blood has always appeared to me obvious and unequivocal. *Dr Davy* says the change is more apparent than real, is produced by the blood being converted for a time by the agitation into bloody froth, and gives place after repose to the original dull purple colour peculiar to venous blood. I have not been able to observe what is here mentioned, provided the blood used was fresh and not decayed. The purple venous blood always became by agitation with air brightly crimson, and remained so for more than a day, sometimes for four days; and the difference in tint was so great that no one could mistake the two varieties of blood at the distance even of five-and-twenty or thirty feet.

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* Mémoires de la Société d'Arcueil, ii. 462.

† Edin. Med. and Surg. Journal, October 1830. *Dr Davy's* paper was read before the Edinburgh Medico-Chirurgical Society early last winter.

Dr Davy adds, that agitation in hydrogen gas had the same effect on venous blood as agitation in atmospheric air. But in this respect also my experiments differ from his. When I agitated venous blood in hydrogen, which had previously been entirely freed from oxygen by a ball of spongy platinum, no change whatever could be remarked in the colour; and when the same blood was then agitated in atmospheric air, it became bright crimson as usual. In all the experiments I have been mentioning, the blood was drawn not more than three hours, and sometimes only one hour, before it was used.

There appears to me therefore no reason for doubting that venous blood acquires the colour peculiar to arterial blood, after being agitated with air out of the body.

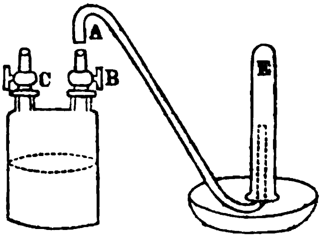
II. A much more interesting department of this inquiry relates to the alterations which the air undergoes in composition during the changes effected in the colour of the blood. It is generally conceived that the brightening of the colour of the blood is connected with the disappearance of oxigen and formation of carbonic acid. But *Dr Davy* has apparently rendered it probable that these alterations of the air are not produced by recent blood, and, where they were observed by former experimentalists, must have arisen from the decay of the blood, in the same manner as they might arise from the decay of any animal matter whatsoever.

I am sorry, however, that my experiments compel me again to differ from so eminent and accurate a chemist. I have tried the effect of agitating venous blood in air no less than thirteen times, in expectation of meeting with some fact which might reconcile my previous observations with those of *Dr Davy*, but I have not been able to remark in a single instance that want of action on the air which he believes he has established. I shall describe the experiments here alluded to in detail. For they seem to afford more precise information than any hitherto possessed respecting the arterialization of the blood out of the body; they prove completely that the process of arterialization, so far as regards the changes which the blood undergoes in colour and the air in composition, is a chemical and not a vital phenomenon; and they will contribute afterwards to explain some doubtful points in the physiology of the blood, which it is otherwise extremely difficult to comprehend.

For the following experiments the blood was prepared by removing its fibrin without the contact of atmospheric air. This was done by filling a bottle to the lip from a large orifice in a vein, dropping in some bits of lead, inserting a glass-stopper with a small groove in its side, and shaking the lead in the bottle. In this way the lead collected all the fibrin with very little of the colouring matter, while no air got into contact with the blood after it was drawn, except a few bubbles, which entered by the groove on the stopper as the blood cooled.

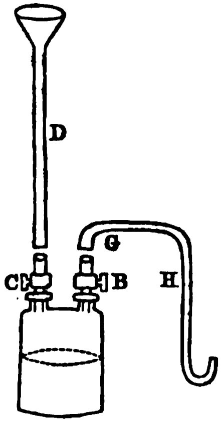
The mixture of serum and colouring matter was then cooled to the temperature of the apartment, which in different experiments varied from 44° F. to 52°; and when this was exactly attained, the mixture was quickly introduced into a graduated bottle of the capacity of twenty-three cubic inches. Ten cubic inches of the mixture were commonly used, and the agitation was performed sometimes an hour after the blood was drawn, but never later than three hours. The agitation was continued for two minutes, care being taken all the while that the bottle should not be heated by the body of the operator.

In every experiment of this kind I found, that the air was sensibly diminished in volume. The diminution and its exact amount are not very easily proved: at least they cannot be shown by the ordinary mode of levelling in a vessel of water, for the frothy surface remains on the blood for days, and prevents an accurate measurement. But the objects in view may be attained by the following contrivance. The bottle I employed had two mouths, each accurately fitted with a tube and stop-cock. Over the stop-cock B a small curved tube A was fitted by grinding; and this terminated within the graduated glass jar E, which was nearly filled with air, and rested on a saucer of water. When, after agitating the bottle and fixing the tube, the stop-cock B was opened, the other remaining closed, the water rose in E; and the rise, properly measured by levelling the inner and outer surfaces of the water, gave the amount of the absorption. I should add, that care was taken to ascertain by experiment that the temperature of the contents of the bottle was not increased



by the agitation. The increase did not exceed the third part of a degree of Fahrenheit, which was too small a quantity to affect the results.

This is a very satisfactory mode of exhibiting the simple fact and amount of the absorption. But as it was attended with the disadvantage of introducing fresh air into the bottle, and therefore complicating the subsequent analysis of the air, another method was resorted to, by which the air within was both measured and removed for analysis in a state of purity. Instead of the tube already described, another tube G of small diameter, and with a perpendicular limb H, was fitted to the stop-cock B. This tube terminated under mercury, which consequently rose in it when the stop-cock B was opened; and the rise in inches, compared with the atmospheric pressure at the moment, pointed out by the usual calculation the volume of residual air in the bottle at the density of the surrounding atmosphere. In this manner the absorption could be accurately ascertained. As soon as the rise of the mercury was measured, a tube D, terminating above in a funnel, was fitted on the other stop-cock C; and the air in the bottle was then expelled through GH, by keeping the tube and funnel D constantly full of water, and opening the corresponding stopcock C. The air being collected over mercury, it was analyzed for oxygen by means of phosphorus, for carbonic acid by means of solution of baryta or caustic potass, and for azote by ascertaining the residue after the previous steps of the analysis. The temperature in the course of these experiments varied from 44 to 52° F.



In every instance, then, a sensible diminution took place in the air in the bottle; that is, some part of the air was absorbed. The extent of the absorption varied in different experiments, for reasons which will be mentioned presently. The extremes were 0.32 of a cubic inch, and 1.14 cubic inch for ten cubic inches of the mixture of serum and colouring matter.

In every instance, too, carbonic acid was formed. This was proved both by the diminution indicated in the graduated tube by the action of caustic potass or baryta, and also more obviously in the latter case by the formation of a precipitate of carbonate of baryta. But the quantity of carbonic acid formed was always greatly less than the oxygen which had disappeared, being never more than 0.25 of a cubic inch; and indeed its quantity was never materially different from this, being never less than 0.20. This small formation of carbonic acid is easily accounted for. The serum of blood, as *Dr Davy* formerly showed, and as I have also ascertained, has a strong solvent power over carbonic acid, so that with the help of a little agitation, it will very soon absorb more than its own volume of this gas. We may be quite sure, therefore, that in the preceding experiments much more carbonic acid was formed than existed in the residual air.

The carbonic acid was always formed at the expence of the oxygen of the original air. This was proved by the circumstance, that the quantity of oxygen in the original air was equal to the sum of the oxygen left in the air after agitation, of the total absorption ascertained by measurement, and of the oxygen in the carbonic acid gas. Sometimes the sum of all these *fell short of* the quantity of oxygen in the original air by a small fraction, such as lay within the limit of error in analyses of this kind. But the sum of the three particulars never *exceeded* the original oxygen.

The quantity of azote in the residual air appeared in general to be exactly the same as that in the air originally. In three experiments the azote in the residual air, and that in the original air, were as follows:

1st,	10.10 cubic inches	10.12
2d,	10.26 do.	10.27
3d,	10.33 do.	10.35

Here then certainly no azote was absorbed or given off by the blood. In two experiments, however, a very small quantity appeared to be given off by the blood. But of this I am by no means satisfied, as the present method of experimenting is on too small scale to justify any safe conclusion regarding such small differences as I obtained.

It is clearly ascertained, however, that, when venous blood acquires the arterial colour by agitation with atmospheric air, a considerable proportion of the oxygen of the air disappears, that carbonic acid is formed, and that more of the latter gas would be discoverable in the air, if the serum of blood did not possess the property of dissolving it. All the essential particulars, therefore, of the arterialization of the blood in the lungs of the living body,—so far at least as relates to the change in the colour of the blood and composition of the air,—take place equally well with blood out of the body; and, consequently, these particulars in the arterialization of the blood are independent of vitality.

I have already remarked, that the absorption which is effected by venous blood when agitated with air, differs considerably in different instances. In like manner, of course, the total amount of oxygen which disappears, including both this absorption, and the oxygen in the carbonic acid, is materially different. The greatest amount I have hitherto remarked, was 1.4 cubic inch for ten cubic inches of blood; the least 0.57 of a cubic inch; and between these extremes it has amounted to two-thirds of a cubic inch, three-fourths, four-fifths, one cubic inch and a tenth, a cubic inch and a quarter.

These differences in the loss of oxygen are much too considerable to have depended on mere differences in the degree of exposure of the blood to the air in the act of flowing from the vein. For I took care never to use blood for the preceding experiments, which had not flowed in a full rapid stream; and besides it happened, for a reason which will be presently obvious, that, in the four instances where the loss of oxygen was least, the blood had been collected with unusual rapidity; and that this was peculiarly the case in the instance where the loss of oxygen was least of all. In short, on the whole, the specimens which acted least on the oxygen of the air in the bottle, had nevertheless been least subjected to partial arterialization during the process of collecting them.

It is worth while to inquire into the real causes of the differences in the loss of oxygen. For they are chiefly certain inherent differences in the state of venous blood, which are indicated indeed by other physiological facts, but by none so unequivocally as by this variety in the power of altering the oxygen of atmospheric air.

The first cause I shall mention is a difference in the degree of venosity or venalization of the blood in passing through the capillaries. That such a difference exists, is partly indicated by varieties in the colour of venous blood, which no medical man can have failed to remark. The usual colour of venous blood while issuing from a vein is dark purple; but in many febrile diseases, where the circulation is much excited, and especially in severe cases of acute rheumatism, its colour is unusually florid; and I have several times seen it issue of so bright a tint, that the operator was for an instant afraid he had opened an artery. This peculiar state of the venous blood will be naturally ascribed to the arterial blood having been less venalized in passing through the capillary circulation. And in general we need seek for no other cause of this inferior degree of venalization, than the unusually great impetus with which the blood is driven. That it does pass through the capillaries with unusual force and celerity in such circumstances is obvious, from the fact, that the stream from the divided vein issues with unusually great force; nay I have sometimes seen it issue *per saltum* when very florid, the impulse of the heart, generally confined to the arteries, being then transmitted through the capillaries to the veins. *

But the varieties thus partly indicated in the venosity of the blood are more clearly established by the differences I have mentioned in its power of altering the composition of the air. Its inferior action on the oxygen of the air in its passage to the arterial state simply indicates, that it is less removed from a state of arterialization, that is, partakes less than usual of the characters of venous blood. Accordingly, the least alteration of oxygen invariably occurs in those febrile diseases where the circulation is much excited, and the respiration at the same time free. These conditions exist most especially in acute rheumatism; and it was therefore in cases of this disease that the four instances of slight action formerly mentioned have occurred. On all these four occasions the blood was evidently more florid than usual, and in the instance where the loss of oxygen was only 0.57 of a cubic inch, the stream from the vein was so bright, that the gentleman who opened it had at first some suspicion that he had opened the artery.

Another cause of diversity in the action of venous blood on atmospheric air is a difference in the proportion of colouring matter contained in the blood.

Every physiologist is aware that the proportion of solid matter contained in the blood, and consequently the proportion of its colouring matter, differ much in different circumstances. This is a subject, however, upon which very little precise information has been collected, and very few experimental inquiries have been made. It is therefore one of the topics which it is my purpose in the present series of investigations to endeavour to elucidate, by establishing what are the conditions of the body in which the colouring matter and other principles of the blood abound or are defective, and what relations their abundance or deficiency bears to diseases and their progress. My experiments in this department of inquiry are still too few in number to admit of being mentioned particularly. But I may anticipate the results which will be stated on a future occasion by observing, that a very great difference does really exist in the proportion of solid ingredients, or in what may be termed the richness of the blood, and that its colouring matter appears to be peculiarly deficient in the advanced stages of fever, and in some forms of dropsy.

This much being stated, I need hardly add that when the colouring matter is deficient, the action of the blood on atmo-

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* The pulsating flow of venous blood may be sometimes owing to the pulsation of the artery immediately under the vein which is opened. But in two of the instances alluded to above, there was no artery near the vein to communicate impulse by mere juxtaposition.

spheric air will be proportionally slight. The following is a good example. In the case of a young woman affected with dropsy from enlargement of the heart, blood was drawn and prepared in the usual manner; but, although the colour of the blood was very dark, the oxigen which was altered by ten cubic inches of the mixture of serum and colouring matter was only $\frac{7}{10}$ths of a cubic inch, or about half the average for healthy venous blood. This result was easily explained by the state of the blood, which contained a very unusually low proportion of colouring matter. I was almost satisfied at the time, from circumstances which it is unnecessary to mention at present, that the blood did not contain the usual proportion of colouring matter. But I have since had an opportunity of ascertaining positively that such was the fact. About a fortnight afterwards the patient was bled again; and on that occasion the colouring matter was found to be somewhat less than six per cent. of the blood, while I have found the average in healthy blood to be at least eleven or twelve per cent. *

Note.—It may perhaps be expected that I should endeavour to account for the failure of Dr Davy to observe any action of venous blood on atmospheric air. This is not very easily done, as he has not described the particulars of his method of operating. But I think the results of some of his experiments are susceptible of explanation. We have seen that the absorption of oxigen varied from a ninth to a thirtieth of the volume of the blood. Now, Dr Davy sometimes operated with a drachm of blood in three or four cubic inches, that is, in twelve or sixteen times its volume of air. Consequently, the greatest possible absorption which could have taken place was a thirty-sixth part of a cubic inch; or between the 108th and 144th part of the volume of the air employed; and it might have been so small as the 120th of a cubic inch, or between the 360th or 480th part of the air. Quantities so insignificant might very easily have escaped notice.

ART. XI.—*Facts and Observations regarding the Treatment of Venereal Diseases.* By WILLIAM DONNELLY, M. D. Surgeon in the Royal Navy.

THE only explanation necessary for understanding the following tables, statements, and remarks on venereal diseases, is, that they are copies of those that accompany the detailed account of the cases in my final journal for the Hussar, to be forwarded to the Medical Commissioners of the Navy on the ship being paid off; the cases stated to have occurred on board the Sparrowhawk having been inserted in the annual journals for that ship, transmitted to the Medical Board in the years 1823, 1824, and 1825.

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* When I repeated the experiment of p. 98 before the Royal Society, I announced that from the appearance of the blood I was afraid the colouring matter was deficient in quantity, and, consequently, that the absorption after agitation would not be so large and obvious as I could have desired. The absorption was not great. Having since analyzed the blood then employed, I find the colouring matter was only eight per cent.

I would not occupy the pages of the Medical and Surgical Journal with individual cases, which, though some of them furnish single facts of interest, are generally such as occur to every surgeon in practice. The general results only, with a view to practical conclusions, I conceive to be important. Those that I have exposed will be estimated at what they are worth; and in them I have chiefly to regret that the general duration of service in one vessel, which rarely exceeds the triennial period, does not afford a longer time for the surveillance of patients after the cure of primary affections.

To furnish data on this important point, I know no class of practitioners so favourably circumstanced as surgeons of regiments, many of whom having served almost constantly with their men for the last ten or twelve years, during which genital ulcers, &c. have in general received local treatment only, could, without difficulty, furnish statements similar to the following, (No. 1 and 2,) but more valuable, as they could show the results of many cases for at least from seven to ten years.

This length of exemption from secondary symptoms, though satisfactory to the majority of observers, will not obtain the belief of others. These ascribing almost all varieties of disease to a latent un-eradicated syphilitic or gonorrhœal virus, will not believe the most solemn asseverations of patients never having had venereal disease; or if compelled to do so, will trace the supposed poison, for its existence cannot be demonstrated, to ancestors, not only of the first, but of the second and third degree.

I remember a remarkable case of excellently characterised syphilitic ulceration of the throat, soft palate, &c. with caries, and finally loss of the bones of the nose and face, which, by the gradual progress of the disease, terminated fatally about six or seven years from its commencement. The girl who was the subject of this had an asylum in the Halifax (N. S.) poorhouse, to which she was admitted in an early stage of the disease, then, to the best of my recollection, between twelve and thirteen years of age, and with a hymen large and entire, and without any genital affection. Neither mercury nor any other means arrested the slow but regular progress of this disease.

If it were syphilitic, whence was the virus derived? And if syphilitic, why not controllable by its assumed antidote mercury? Is it more necessary to believe, without demonstration, that a corrosive sypbilitic poison was in operation here, than it would be to suppose, equally without proof, that a specific poison exists, where we see a common sore on the leg assume an ulcerative process, and spread widely and deeply until the tibia is exposed and begins to exfoliate? Instances might be multiplied of different forms of constitutional, and even local disorders and affections thus proceeding in an unchecked course, yet these are not attributed to the presence of any extraneous poison, though they may be to general enfeebled or morbid constitutional texture or action, either hereditary or acquired.

No. I.

ANALYTIC STATEMENT of Sixty-one Cases of Primary Venereal Affections which have occurred on board H. M. S. Hussar, in England, North America, and at Bermuda, from March 1827 to the end of July 1830.

Classification.	Characters of each class of cases.	No. of each.	No. of days on sick list.			No. under treatment.			Sequela.	Time since well, months.		
			Aver.	Great.	Least.	Aver.	Great.	Least.		Aver.	Great.	Least.
1	Simple gonorrhœa,	4	6	11	2	7	15	2	In one gleet for three days.	21	36	13
2	Do. and bubo,	3	14⅓	19	9	14½	19	9	Slight return of gonor. in one.	11	13	9
3	Bubo not preceded by gonorrhœa or chancre,	3	22	41	7	29⅓	41	7	None.	27⅔	36	13
4	Simple excoriation,	3	2	3	0	2⅔	3	2	In one secondary symptom.	24⅓	36	13
5	Do. with phimosis and gonorrhœa præputialis,	1	9	9	9	9	9	9	None.	8	8	8
6	Do. with buboes and inguinal sinuses,	1	102	102	102	102	102	102	Do.	36	36	36
7	Do. with gonorrhœa, phimosis, and bubo,	1	46	46	46	34	54	54	Secondary symptoms.	0	0	0
8	Chancres only,	17	8⅔	59	0	16	59	4	Do. in two.	21	36	4
9	Do. with gonorrhœa,	1	8	8	8	8	8	8	None.	38	38	38
10	Do. with gon. and excoria.	1	11	11	11	18	18	18	Do.	36	36	36
11	Do. with excoriation only,	3	0	0	0	5½	7	4	Do.	17⅔	36	7
12	Do. with warts,	3	14⅔	34	4	14⅔	34	4	Do.	26½	38	13
13	Do. with bubo, suppurating and non-suppurating,	12	18⅔	40	4	27¼	50	4	Do.	17⅓	36	4
14	Do. do. do.	4	87	130	27	92½	130	36	Do.	26¼	36	16
15	Do. with gonor. and bubo,	2	9	10	8	14½	21	8	Do.	15½	21	10
16	Do. with gonorrhœa, excoriation, and bubo,	2	30½	56	5	30½	56	5	Do.	14	19	9
	Total,	61										

Result of treatment of the above 61 cases, including 4 tedious ones, in which mercury was given, and the one of 102 days, with inguinal sinuses succeeding to excoriation only,	24⅓	130	0	28	130	2		22⅗	38	4
Result excluding these five,	14¼	56	0	18	56	2		22⅗	38	4
Result excluding only the four in which mercury was given,	20	102	0	23½	102	2		22⅗	38	4

Of these cases the general treatment was rest, low diet, abstraction of liquor, and occasionally a saline cathartic. In the buboes, cold applications were used to prevent suppuration; and when this result took place with sinuses, the swellings were opened by incision, and occasionally by caustic. Excoriations were treated by sulphate of copper or nitrate of silver. Warts were removed by the knife, and by nitrate of silver. Seventeen cases of chancre were treated with ordinary dressings, by nitrate of silver, sulphate of copper, or sulphate of zinc occasionally, without particular regimen. Twelve cases complicated with bubo, suppurating and non-suppurating, were treated by confinement and local remedies, as indicated.

Of twenty-six cases of primary venereal affections which occurred in the Sparrow-Hawk, in England, Sicily, and at Naples, in which the general characters were excoriations, chancres, and buboes, suppurating and non-suppurating, and which were treated without mercury, they were at an average fourteen days on the sick list, and eighteen days under treatment; in one only did secondary symptoms supervene two months after the cure of the primary affection; and in the other twenty-five, which were watched for fifteen months, no symptoms were observed to follow during that period.

Of the 61 cases treated in the Hussar without mercury in 4 did secondary symptoms supervene under the following circumstances.

In the first case, excoriation of the *glans* healing in two days was followed after thirteen months by nocturnal pains of the shins and general debility. These were removed by mercury given to ptyalism, but recurred in three weeks and were aggravated by mercurialization. Cinchona in large doses afforded relief; but the patient, whose general health was bad, was invalided.

In the second, gonorrhœa and bubo, with phimosis, requiring division of the prepuce, was followed after thirty-three days by pains of the shins and copper-coloured cutaneous pimples and crusts preceded by fever. These symptoms were removed by mercury given to slight ptyalism, but recurred after twenty-five days, and were permanently removed by a longer continued mercurial course proceeding to severe salivation; forty-one grains of calomel were taken at both times.

In the third case, a foul chancre of the *frænum*, succeeded by inguinal swelling, was followed after 106 days by cutaneous pimples and crusts, and pain in the shins, all of which readily yielded to a slight mercurial course.

In the fourth, two chancres of the *corona glandis*, one foul, were in forty-five days followed after two days of fever, by cutaneous pimples and crusts with general pains. These symptoms were removed by the exhibition of thirty-three grains of calomel, which slightly affected the mouth, but recurred in three weeks, and were permanently removed by the administration of ten grains more.

Lastly, among the whole number of cases treated in both vessels, I met with six cases in which secondary symptoms appeared without being preceded by those which are regarded as characteristic of syphilitic action, or after those symptoms were supposed to have healed entirely.

In the first, a person of great probity, who had been married twelve years, and never had any venereal affection, had deep, excavated, and spreading ulceration of the throat. After the repeated local application of nitrate of silver, sulphate of copper, &c. during a space of forty-nine days without benefit, this ulceration healed permanently under a steady and protracted course of mercury. In the second case, in which the patient had gonorrhœa nearly six years before, and had been severely salivated at least six times in the two previous years for rheumatism, painful nodes appeared in the sternum, and nocturnal pains in most of the bones. These derived only temporary relief from cinchona and calomel in small doses; and the patient was invalided. In the third, in which ptyalism had been three times induced for chronic disease of the *testes*, nodes supervened a few weeks after the last salivation, and mercury was again exhibited, so as to induce severe salivation twice, in combination with sarsaparilla. In the fourth, in which salivation had been repeatedly induced by the use of mercurial cathartics and mercury for the removal of stricture, gonorrhœa, and rheumatism, nodes with cutaneous pimples, crusts, and ulcers supervened, and were benefited by mild diet, sarsaparilla, and at one time minute doses of blue pill.

In the two last there had been chancre, in both removed by a mercurial course. In one, after ninety days, nodes and pains appeared, were removed on the induction of salivation, but recurred in forty-nine days as severely as at first, and were permanently removed by *cinchona* alone. In the second, seven years after the primary symptom, and at least three salivations, ulceration of the throat, and nodes, and nocturnal pains supervened, and, though alleviated by the use of mercury, were finally cured only by *cinchona*.

No. II.—Table of Cases of Syphilis treated at Bermuda Naval Hospital.

Years.	Total admitted.	Cured.	In this average no. of days.	The greatest no. of days of any.	The least no. of days of any.	Invalided.	After this average no. of days.	After the greatest no. of days in any.	After the least no. of days in any.	Dead.	After this average no. of days.	After the greatest no. of days in any.	After the least no. of days in any.
1813	25	17	69	177	20	6	$110\frac{1}{3}$	228	36	2	46	81	11
1814	19	14	$48\frac{1}{3}$	215	18	5	$75\frac{2}{3}$	109	37				
1815	9	4	$29\frac{1}{2}$	39	12	5	$30\frac{2}{3}$	58	11				
1819	9	8	$32\frac{1}{4}$	57	3	1	37	37	37				
1820	4	4	46	83	12								
1821	4	4	$72\frac{1}{2}$	148	35								
1822	3	3	78	148	28								
1823	9	7	36	90	24	2	140	250	30				
1824	12	12	$44\frac{1}{4}$	96	7								
1825	6	6	33	68	13								
1826	3	3	46	69	17								
1827	9	9	$44\frac{2}{3}$	90	15								
1828	16	16	24	63	1								
1829	8	7	$65\frac{1}{2}$	103	25					1	19	19	19
Total,	136	114	$47\frac{1}{3}$	215	1	19	$78\frac{1}{2}$	228	11	3	$52\frac{1}{2}$	81	11
Proportion,		$\frac{57}{68}$			nearly	$\frac{1}{7}$				$\frac{1}{45}$			

No. III.—Table, similar to the above, of Cases of Syphilis treated at Halifax Naval Hospital.

1809	31	27	$95\frac{1}{2}$	374	20	2	83	107	59	1	124	124	124*
1810	13	11	100	467	21	2	153	300	6				
1811	13	13	49	130	15								
1812	23	20	108	124	16	3	160	240	96				
1813	51	25	$33\frac{1}{2}$	119	8	25	$15\frac{1}{2}$	47†					
1814	34	25	42	150	11	8	43	134	4	1	14	14	14
1815	15	14	56	210	7	1	4	4	4				
1816	12	10	$76\frac{1}{4}$	300	16	2	7	14	1				
1817	10	5	56	112	27	5	237	910	31				
1818	9	7	23	35	9	1	288	288	288	1	72	72	72
1819	1	1	29	29	29								
Total,	212	158	61	467	8	49	121	910	0	3	70	124	14
Proportion,		$\frac{3}{4}$		Be	tween	$\frac{1}{4}$ & $\frac{1}{3}$				$\frac{1}{71}$			
Total of both,	348	272	54	467	1	68	100	910	0	6	$61\frac{1}{4}$	124	11
Propor of both.		$\frac{68}{87}$			nearly	$\frac{1}{5}$				$\frac{1}{58}$			

* One ran from the Hospital. † One sent to sick quarters.

In my three former annual journals for this ship, I have intentionally omitted to insert any venereal cases, reserving them until the ship should be on the point of being paid off, to be enabled to state the results and sequelæ of the different primary affections when treated, as I generally have treated them without mercury.

From the daily memoranda kept of these cases, I have given as succinct an account of each as is consistent with perspicuity; and, to give a condensed view of the whole, I have drawn up a tabular statement in which the primary affections, in their various forms, are classified as far as their varying nature and combinations permit; their general treatment indicated; the average, greatest, and least number of days of each on the sick-list, and under cure shown; their sequelæ noted, and the length of time the patients were known to have remained subsequently well specified. From this a general average of the length of time on the sick-list, and under cure, is calculated, as well as of the time elapsed without developement of secondary symptoms.

The particular circumstances of these may be seen on inspecting the first table and perusing the general statement (p. 106,) of 26 cases of excoriation, chancre, and bubo, that occurred during three previous years that I was surgeon of the Sparrow-hawk. These cases were treated without mercury.

This practice, which I then adopted, I have since continued to employ with the view of determining, as far as my opportunities, with close and unprejudiced observation, enabled me, whether the cure of primary venereal affections require the use of mercury, and is generally expedited by its action,—and whether the developement of secondary symptoms be a more frequent occurrence when mercury was not at first administered. I was prompted to undertake this inquiry from the fact, that, however decided were the opinions and practice of the army-surgeons on these points, in the navy no conclusion seemed to be established, partly because it was conceived impracticable to imitate, under the peculiarities of the naval service, the proceedings in regimental hospitals; and because it is only under particular circumstances that the surgeon of a ship or naval hospital can do otherwise than act in conformity with established treatment.

In the navy, indeed, the practice of administering mercury is so uniform that it rarely becomes a question whether it is necessary in any individual case; and the patient is forthwith subjected for every species of genital affection succeeding to sexual intercourse, gonorrhœa by some not excepted, whether recent or remote, to a mercurial course, to be renewed if the inguinal glands become subsequently affected, and repeated again and again if other symptoms appear, or the original affection return, until at length the surgeon becomes embarrassed, and, not knowing whether he has to treat a mercurial or a syphilitic disease, finds it necessary to recommend change of air, decoction of sarsaparilla, and alterative doses of blue pill.

With regard to secondary symptoms, or those cases with characters correspondent to the definition of syphilis, in addition to the more detailed statements in the respective cases, I have given short statements of all such as have occurred during my period of service in the Hussar and Sparrow-hawk. These may be divided into those that have occurred subsequently to primary affections treated without mercury; those treated by the use of mercury; secondary symptoms without the precedence of primary; and the same after the use of mercury in other diseases;—to illustrate the circumstances under which the symptoms characteristic of secondary syphilis occur.

There are in all ten cases to which the character of *secondary* is applicable. Of these four occurred after primary affections, one only excoriation, one the same, with gonorrhoea and bubo, and two simple chancres treated without mercury. After the lapse respectively of 33, 45, and 106 days, and of 18 months, one of them was invalided, and the others were cured by very measured courses of mercury. Two were subsequent to chancre in one, and chancre and bubo in the other, treated by protracted mercurial courses, appearing in the first three months, and in the second, according to the patient's own statement, seven years subsequently. Three were consequent or repeated and severe salivations in each for the cure of rheumatism, and other complaints not syphilitic; and one was without the precedence of any venereal disease or mercurial treatment. From these facts it may be justly inferred, that the assemblage of symptoms denominated *secondary* may be the consequence of the influence of mercury as readily as of the syphilitic poison.

Whether the occurrence of secondary symptoms, with one exception of a slight and tractable nature, in five cases out of eighty-three, be an equal, or greater or less proportion than obtains where mercury is systematically exhibited for the primary affection, can only be accurately determined by those who, invariably giving mercury, carefully noted this circumstance, and attended to the character of the cases of secondary disease which presented to them. Memory will recal to every old surgeon numbers of aggravated cases of secondary disease subsequent to the most liberal use of mercury in the primary affection.

Respecting the time required for the treatment, and the consequent length of exemption from duty, when mercury is used and when mercury is not used in the cure of primary symptoms, a near approximation to the truth may be made by comparing the tables of cases in the Hussar and Sparrow-hawk, with the two tables that I have given of Bermuda and Halifax Naval Hospitals for several years.

These tables show that of 348 cases 272, or rather more than three-fourths of the whole, were cured; whilst sixty-eight, or nearly a fifth, were invalided, and six died. The whole being cured, invalided, or dead after an average of sixty-one days in the hospitals, consequently, after being between three and four times as long on the list, and very nearly three times as long under treatment as were the cases in the Hussar and Sparrow-hawk, all of which were cured on board, except three which were sent to hospitals, two of which are included to their cure in the average time, but the third, having been sent to an hospital in England, and being complicated with other disease, the result remains unknown.

Though during most of the years included in these hospital returns, it was customary to send patients generally with venereal diseases to hospitals, for the purpose of being submitted to a mercurial course, yet it is to be supposed the slighter cases were not sent; and allowance, therefore, is to be made for the others being longer under treatment. At the same time, it is to be remembered, that all the cases sent to the hospitals must have previously been on the sick-list on board a longer or shorter period, of which no account is taken in the time they were under cure.

At Bermuda Hospital also, for the last seven or eight years at least, most of the venereal cases received were from the troops stationed on Ireland island, and included those of every character, sometimes even gonorrhœa. It is true also, that I have placed amongst the cases in the Hussar four with gonorrhœa, and three with gonorrhœa and bubo; but it will be seen that the average of the first is about the same, and of the second, much more than that of simple chancre, whilst that of three of simple bubo is three times as much.

If then the data I have furnished are considered sufficient to warrant any deduction, I can only conclude, that, as far as shortening the time of cure is important, the advantage is much in favour of the non-mercurial treatment, which is at least gratuitous, where sores heal without its aid, whilst the inconvenience to the patient and service is infinitely less.

With regard to secondary symptoms, I am inclined to think that five out of eighty-one is a smaller proportion than would have followed full mercurial courses in them all, or perhaps would succeed to such a liberal use of mercury in any equal number of persons as happened to some of the secondary cases of the Hussar and Sparrow-hawk. As in all the others, from eighteen months to two years, have on an average elapsed since their cure, I see little reason to apprehend the developement of any sequelæ, considering also, that in the five cases in which secondary symptoms have occurred, the average time subsequent to the primary disease was only four months and a-half.

H. M. S. Hussar,
26th July 1830.

Art. XII.—*Extracts from a Correspondence on the Filaria medinensis among some of the Medical Officers in the Honourable East India Company's Service at Bombay; with a Letter from* Dr Robert Grant, Professor of Comparative Anatomy in the University of London.

About a year ago, a box, addressed to us, containing specimens of the *Filaria medinensis*, and some relative papers, sent from Bombay, was seized on board a vessel which had arrived from the East Indies at Liverpool, and sold at the Custom-House as unclaimed property. It was most obligingly forwarded to us by the gentleman by whom it was purchased.

The papers chiefly relate to the obsolete opinion, that the *Filaria medinensis* is not an individual animal, but a dead portion of lymphatic vessel, which has been for some years back revived at Bombay, and has received the sanction of some of the higher medical officers at that Presidency. This opinion has already been noticed in the Transactions of the Medical and Physical Society of Calcutta, (Vol. I.) and facts there detailed, which, in our opinion, completely overthrow it. We are, however, induced to select for publication the following paper by Dr Milne, superintending surgeon, Bombay, as containing the best account we have seen of the plausible arguments by which the heterodox doctrine is attempted to be supported, and as introductory to their total subversion by Dr Grant.

"*Summary of Remarks on Dracunculus, also termed Lymphatitis.*

"Parts or substances which are equal to or resemble in all respects another part or substance, whose properties and functions are known and admitted, must be the same in property and function.

"The substance which is protruded in ordinary case of dracuncle, is a slender pellucid tube, which varies in size from the thickness of a horse hair to that of the bass string of a violin. At both extremities it terminates in a sharp point, and often presents one, and sometimes two, very slender appendices, like the continuation of radiated vessels, which seem as if they had been ruptured by the force so generally used in extracting it. It is more frequently found larger in Europeans than in natives, probably because their vessels are often larger, partaking in its texture of the character of the subject from which it is extracted, that is to say, firm in those of strong fibre, and loose in those whose stamina are weak. It is a hollow tube, and smooth in surface both externally and internally, and its contents can be pressed, when it is extracted entire, from one end to the other, marking distinctly a pervious carrying vessel, without the slightest indication of animal organization. This tube I have never found deeper than the cellular membrane; and if it has not been separated by effusion or suppuration from the parts in which it is superficially imbedded, it is found to have a firm adhesion to these surrounding parts, of which it seems a constituent portion. If those parts have not been irritated to increased action or inflammation in attempts to extract it, it remains without any material change sometimes for a week or more. If irritation of the neighbouring parts ensues, and if effusion along the course of this vessel takes place, it is at once easily separated, and on some occasions withdrawn entire, bringing with it portions of cellular membrane, and it is then always in a tense and thickened state; when its contents are limpid, or slightly pellucid, exactly resembling those of a lymphatic vessel in a state of health; if the vessel has been ruptured, this degree of violence, added to its retraction, and the discharge of its contents under the integuments, produces effects similar to those of an injection in a case of hydrocele; inflammation ensues, by which, if kept moderate, and if it terminate in resolution, this vessel is absorbed or obliterated; if violent suppuration ensues, and the substance is discharged, it is then always white and flaccid, and both it and its contents seem now to have undergone the same change with the neighbouring parts. These contents are then white and milky, and the vessel can at this period be easily separated into two coats, the inner being the thinner, which can be drawn from the outer like a finger from a glove; and the thickness of these coats is a further proof that such could not be expected to be found in an animal so slender, whose great length, compared with its thickness, also precludes such an idea. This vessel, therefore, is evidently subject to the same laws and changes which affect the neighbouring parts. It is stationary while they are not irritated; its action increases with theirs; it inflames with them, and to the same extent; whether that be violent or moderate, it suppurates with them, and it sphacelates with them; and further, medicine, which acts powerfully on the habit, as emetics, has an evident effect in influencing the tense state of this vessel, speedily rendering it loose and flaccid. Hence it follows, that, being subjected to the same laws and changes which affect the part in which it is found, and corresponding with perfect exactness to a class of vessels which are known to occupy such parts both in its size, structure, and contents, it necessarily follows, that it must be a component part of the subject in which it is found, and can only be one of that particular class of vessels to whose description it exactly answers. Its dimensions are the same, that is to say, never exceeding those of a lymphatic vessel; its contents, when recent, are the same; its coats the same; its length various, as these branches vary. Its course also is generally the course of the principal branches of the limb, viz. the neighbourhood of the ancle, (and in that line upwards,) where constant motion, the sharp angle and depending situation, favouring in hot weather a collection of fluid, and the over-distension of weakened and relaxed vessels, in men of a lax or enfeebled habit of body, or reduced by former illness; and in those laboriously occupied or harassed by fatigue by a long journey, or by any irregularities, and whose digestive organs may have been deranged, irritation ensues with increase of action in the vascular structure of the vessel, tension, effusion under the skin, and vesicle, producing the affection in question, accompanied also on many occasions by phlegmon and nettle rash, which are its frequent concomitants.

" It follows, therefore, from an admitted axiom, that the substance in question cannot be a worm, because its situation, functions, and properties, are those of a lymphatic vessel, and hence the idea of its being an animal is an absurdity.

" But three important practical and pathological facts also tend greatly to elucidate, if not to establish, the character of the affection which is here described.

" These are, ' That where this disease is found very generally to prevail, any other constitutional disorder is but rarely present, and when such does occur, it is for the most part slight.

" ' That when this affection appears among men enfeebled by former disease, it may be considered a favourable omen of returning general health.

" And, ' That, from the indiscriminate prevalence of this affection and of phlegmon, on many occasions in the same corps and situation, and under the same circumstances, it is to be inferred that their origin and cause are the same.'

"Hence from these premises, it may be deduced, that the affection in question is connected with some 'derangement of constitution,' and in ascertaining this point, we establish a train of determining its real nature.

"It is known where phlegmon has become a prevailing affection, that it is seldom that any other formidable disease is found to appear, and when in disordered or diseased constitutions this affection has begun to show itself, it may be deemed a favourable constitutional change. It is even indeed proverbial 'that boils are healthy,' that is to say, 'an indication of a lesser degree of constitutional derangement.'

"It is also observable, where the affection in question is the prevailing disease, that no other endemical disorder is found, and, as this affection frequently occurs in an epidemic shape, it must be subject to the same laws producing epidemic diseases, 'which have their origin in certain modifications of climate, acting on particular constitutions, produced by different localities and habits.'

"This law or principle, therefore, and the analogy between phlegmon and dracuncle, are conclusive of constitutional dependence; and when it can be explained what the circumstances are, that determine in the one instance to phlegmon, an affection of the skin, and cellular membrane, I anticipate that it will not be difficult to elucidate the causes which produce in the other, the disease in question, affecting a neighbouring tissue; and, as the position referring to 'constitutional derangement' is clearly established, the disease termed dracuncle can obviously only be referred to vessels forming the 'superficial lymphatic tissue,'—an affection incident to hot climates only, and to the hot periods of the year in those climates; and further, it is by this reference alone that the phenomena of this disease can be rationally and distinctly explained, and reconciled to known and general principles.

"The subjoined extract, moreover, from the work of a celebrated anatomist and physiologist, Cruickshank, contributes also to explain more clearly these phenomena.

"'The cutaneous veins serve to transmit the blood to the heart under the circumstances of the violent and continued action of the surrounding muscles of the limbs, when, of course, the deep-seated veins must necessarily be compressed, and the circulation of the blood through them be nearly altogether prevented. The cutaneous veins also serve other purposes, *for we find them transmitting a larger proportion of blood during the summer, whether the muscles are in action or not*; and they may serve other purposes yet unknown.' For similar reasons, I have not the least doubt that the superficial lymphatics are *under the same influence*. Hence, during such periods, and under such circumstances, may be inferred their greater liability to irritation and its consequences, increased pellucid arterial action of the substance of these vessels, tension, effusion, and vesicle, which are often followed by inflammation and its effects, in greater and less degrees, during such periods, and under such circumstances, and these consequences are sometimes general throughout the whole length of the vessel, sometimes partial. JNO. MILNE.

" *Severndroog*, 1*st Jan.* 1824."

The whole of the papers, together with the specimens of the worm which accompanied them, were put into the hands of Dr Grant, from whom we received the following valuable letter, which completely sets the question at rest, and contains suggestions for the further investigation of the economy of this curious animal.

" DR DUNCAN,

" MY DEAR SIR,—I regret extremely that many unforeseen avocations have so long prevented me from examining and returning to you the interesting specimens of *Filaria medinensis*, you had the kindness to submit to my examination. I now return the whole of the specimens, together with the documents of the medical gentlemen in India, relating to these animals.

" Notwithstanding the ingenious remarks contained in the accompanying papers, as to these filaments being only diseased absorbents or veins, I have no hesitation in pronouncing every one of them to belong to the common species of Guinea-worm (*Filaria medinensis.*) They all present the usual smooth lengthened cylindrical body, of a whitish yellow colour, tapered to a point at the mouth and anus, and of a firm elastic texture. The outer covering is colourless, transparent, elastic, and almost homogeneous, but we can perceive within it the transverse and longitudinal fibres corresponding to the two muscular tunics pointed out by Cloquet in the Echinorhynchus. The opacity and white colour of the worms is chiefly owing to the absorbed matter which fills the straight intestinal canal, and to the distended state of ovaria in some, and the testicle in others.

" In making a small incision into the ovarium in some of the specimens, I obtained only a multitude of very minute gelatinous globules of various sizes. In others I found the same part filled with incalculable numbers of perfectly formed young individuals, giving a fearful idea of the fertility of these destructive and dangerous animals, while they confirmed the fact of their being at least occasionally viviparous. I showed Dr Christison, before my leaving Edinburgh, a sketch of one of these minute beings as they appeared to me when examined under a powerful microscope. It was almost impossible to distinguish them in that state from the common *vibrios* of stagnant waters. From their prolific nature and their viviparous mode of generation, it is obvious that, by breaking one of them while extracting it from the leg, it might fill the ulcer with thousands of young *Filariæ* already distinctly formed.

"It is stated in one of the papers* that the young when first

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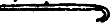
* The paper here alluded to is from Mr Michie at Poonah, and contains some interesting observations.

"Private Dewjee Metta, admitted on the 4th inst. a stout healthy-looking young man, had a hard phlegmonous tumour on the left inner ancle. He was ordered some laxative medicine, and a poultice was laid over the swelling, which was continued till this morning, when, fluctuation being perceptible, the tumour was opened, and upwards of two ounces of good pus streaked with blood were discharged. The wound was simply dressed. After my morning visit, (7 o'clock,) I had not been more than five minutes gone from the hospital, when this man observed a small vesicle about the middle of the right thigh, and directly over the sartorius muscle, which having scratched he destroyed the cuticle, and underneath discovered a whitish substance, which he showed to the native assistant, who knowing what it was, laid hold of it, and pulled out upwards of eighteen inches without the slightest resistance, which he wrapped up in a piece of paper and sent to my house,—a standing order.

"Having a morning call to make, it must have been at least forty minutes extracted before it came into my hands. On opening the paper, I found a very tense and sound-looking substance; both ends appeared quite complete, one much more pointed than the other, and resembling a small hook, (so ). I was looking at it through the microscope, and observed a reddish spiral line extending throughout its greater portion. With a pin I wished to ascertain whether this could be separated from it or not. On applying the point of the pin to it, to my utter surprise, the dracuncle, throughout its whole length, was immediately thrown into the most violent contortions, and one end of it was moved upwards of six inches from the part of the paper on which it lay during the examination. I looked at my watch, and in the space of ten minutes, this motion, equally strong and decided, was repeated four times. In five minutes more, not being repeated, I put it into a phial of clear water, when again the motions were several times most distinctly repeated, and the end with the hook, which sometimes became straight, was moved to several directions in the glass.

"It had not been many minutes in the water, till, from a small orifice in its side and probably near its middle, a whitish (milky,) looking substance, was ejected with considerable force—I say force, because it did not descend directly to the bottom, but was propelled across the phial, as if from a squirt.

"On applying my glass to ascertain the nature of this substance, I was equally surprised to behold that it constituted an innumerable mass of animalculæ, apparently about two-eighths of an inch in length, shaped like a tadpole; the head roundish, very white and opaque; the other part much more transparent. They moved rapidly through the water, and appeared very vigorous. During the whole of this time my attention was wholly absorbed by what was before me, nor did it sooner occur to me how proper it might be to call another person to witness this spectacle. I now sent for Mr Anderson, (about 9 o'clock, A. M.) and, without telling him my motive, I requested him to examine the contents of the phial with my glass. He immediately observed, 'this water is full of animalculæ.' No motion of the dracuncle was observed after his arrival, but he beheld tens of thousands of the animalculæ ejected produced have the form and motions of very minute tadpoles. It would be interesting, in our present uncertainty regarding their origin in the human body, to try whether, in this early stage of their being, they could be reared in water taken from foul stagnant pools in the warm climate of India. In all those I examined under the microscope, the anterior extremity had the same cylindrical form as the rest of the body, and none had it globular like that of a tadpole.

"I cannot help regarding the controversy among our zealous countrymen in the East as to the nature of these animals, in the same light as Rudolphi did many years ago. 'Nostris temporibus Filariam istam animatam in dubium vocari posse, fere omnem fidem superat, et extiterunt tamen qui illam nil nisi cellulosam vel venas emortuas esse contenderint.' *Rudolphi Entoz. Syn.* 1819, p. 207. And I think that their attention would be more advantageously directed to their developement within the ovarium of the worm, and in various media after their expulsion,—to their origin in the human body,—to the most efficacious means of hastening their expulsion from the wound or destroying them within it."

To the foregoing account may be added the fact, that the organic structure of the Guinea worm, as an animal body, has been recently demonstrated by M. Gintrac, in a memoir presented to the Society of Bourdeaux in 1819. From the researches of this author, it results that the Guinea worm presents a head with a sucker, an unciform tail, and an intermediate body consisting of a series of alternate swellings and contractions, distinguished by transverse *rugæ*; that it has no distinct internal canal, but that it consists of pulpy matter, with numerous minute vessels not ramified, but convoluted, and which M. Gintrac, therefore, regards as simple canals. From the testimony of Richard, to whom we are indebted for this notice, these canals were distinctly seen by the members of the commission; and the vermicular motion of the animal was recognized among the muscles before its removal from the body. *

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from the sides of the dracuncle at several different parts, and exactly in the manner already described. They continued to be thrown out at intervals for the space of half-an-hour, and the whole phial of water appeared a complete animated mass.

"Though less numerous than in the morning, they are still (4 P. M.) moving about with great rapidity. So much for dracuncle containing living animalculæ. I shall from time to time watch their progress, and, should any thing worth notice occur, report the same to you."

* Journal de Pharmacie, No. xx. October 1830. P. 628.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Dublin Medical Transactions. A series of Papers by Members of the Association of Fellows and Licentiates of the King and Queen's College of Physicians in Ireland.* New Series, Vol. I. Part I. Leckie. Dublin, 1830. Pp. 388. 8vo.

THE Physicians of Dublin, as the work now before us will testify, continue to display great zeal and activity in communicating to the public the more important and novel results of their experience. The present volume contains eighteen papers on subjects connected chiefly with symptomatology, pathology, and obstetrics; and we may safely say that several of them will be considered very valuable contributions by every reading practitioner, while, with the exception perhaps of a single article, every one possesses sufficient novelty or interest to deserve a place in the collection.

Under the head of Symptomatology, we find observations on recovery from ruptured uterus by *Dr Collins*,—on the application of the stethoscope to the discovery of pulmonary apoplexy, by *Dr Ferguson*,—on hematemesis connected with diseased liver, by *Dr Law*,—on hydrophobia by *Dr Purdon*,—and on polypus of the heart by *Dr Harty*. The pathological papers are on melanosis by *Dr Crampton*,—on perforation of the stomach, and the escape of a lumbricus through the hole, also by Dr Crampton,—on putrefactive disorganization, or gangrene of the lungs, by *Dr Law*,—on uterine cancer, by *Dr Beatty*,—on extra-uterine pregnancy, by *Dr Collins*,—on ovarian tumour, by *Dr Montgomery*,—on general disorder of the cavities and valves of the heart, by *Dr Clinton*. In Obstetrics there is an essay on the application of the stethoscope to the discovery of pregnancy by Dr Ferguson,—on the safety of the forceps in midwifery practice, by *Dr Beatty*,—on a rare species of labour, by

Dr Ferguson,—and on a rare malformation of the fœtus, by *Dr Montgomery*. Lastly, there is an elaborate statistical report by *Dr O'Brien* on the diseases of Dublin in 1829, more particularly on fever; and a physiological disquisition by *Dr Corrigan*, on the motions and sounds of the heart.

We shall commence the present notice with a sketch of the observations on the movements and sounds of the heart, by *Dr Corrigan*, whose views have attracted a good deal of attention on account of their novelty, plausibility, and total departure from pre-established doctrine.

Laennec, it is well known, taught that the impulse of the heart, felt in the left side with the hand or stethoscope, concurs with the contraction of the ventricles, and is synchronous with the arterial pulse; that the first of the two sounds, heard with the stethoscope applied over the region of the heart, coincides with the contraction of the ventricles, and the second with the contraction of the auricles; and that these sounds are produced in all probability by the muscular contraction of the walls of the respective cavities. It is likewise familiarly known, that the most current opinion, as to the cause of the impulse of the heart against the side, coincides with the statement of Hunter, originally made by Senac,—that it is produced by the tendency of the current of blood to straighten the arch of the aorta in passing through it; for if the impulse given to the blood in the arch has such a tendency, it is obvious that the moving power must react on the heart, since the aorta itself is fixed. Such are the particulars which it is necessary to bear in mind in order to understand Dr Corrigan's observations.

The author disputes the accuracy of almost every article of the principles now laid down. He maintains that the impulse of the heart against the inside of the chest is not produced by the tendency of the aorta to straighten itself,—that the arch of the aorta has no such tendency,—that the heart's impulse is produced by the contraction of the auricles, which dilates the ventricles, and so raises the apex of the heart against the side,—that the impulse consequently does not coincide with the contraction of the ventricles, but with that of the auricles,—that the impulse is not synchronous with the arterial pulse, but precedes it,—and that of the two sounds of the heart, the first, or dull sound, corresponding with the impulse, accompanies the contraction of the auricles, while the second or sharp sound,—which Laennec ascribed to the contraction of the auricles, but which, according to Mr Turner of this place, accompanies the dilatation of the ventricles,—coincides neither with the one nor with the other, but with the contraction of the ventricles, and is produced by their internal surfaces striking one another forcibly at the close of each contraction. These are all very important views, in so far as they bear pointedly on the diagnosis of diseases of the heart by means of the stethoscope. Let us see how far he is borne out in them by the arguments adduced in his paper.

His observations may be conveniently arranged under two heads. One head includes his statements relative to the rhythm, or order of arrangement, of the heart's sounds, impulse, and contractions, together with the impulse of the arterial pulse. The second comprehends an inquiry into the causes of the heart's impulse and sounds.

1. The want of synchronism between the arterial pulse, and the heart's impulse felt in the left side of the chest,—the priority, in short, of the latter by a sensible interval;—is the fundamental fact from which *Dr Corrigan* was led to deduce his ideas regarding the rhythm of the heart's sounds and motions. If one hand, says he, be laid on the side where the apex of the heart beats, and the other on the pulse at the wrist, the impulse of the heart will be felt to precede the pulse by a sensible interval, especially if the heart is beating slowly. Now, continues he, this is incompatible with the common notion, that the impulse of the heart is synchronous with the contraction of the ventricles. For the pulse is not only unequivocally owing to the contraction of the ventricles, but is likewise synchronous with it to an instant; because, according to a law in hydraulics, the arteries being always full of blood, the column of blood in them receives at the same instant, at its most distant extremity, any impulse communicated at its commencement. Since the arterial pulse, then, indicates the precise moment of the contraction of the ventricles,—it follows that the impulse of the heart, as it sensibly precedes the pulse, must be owing to something else besides the contraction of these cavities. Accordingly, on laying open the chest of living animals, he remarked, as well as several of his friends who witnessed the experiments, that the impulse accompanied not the ventricular, but the auricular contraction; and the order of the phenomena, after repeated careful observations, appeared to be this,—1. Auricular contraction with the impulse; 2. Followed by ventricular contraction with great rapidity, or as it were through a propagation of muscular action from the auricles to the ventricles; 3. Correspondence of ventricular contraction with the pulse; 4. A short repose of the whole heart. When the sounds of the heart were observed at the same time, it appeared that the auricular contraction and impulse were attended with the slower dull sound remarked with the stethoscope on the chest, and the contraction of the ventricles with the shorter, loud sound; and the relative shortness and loudness of the two sounds corresponded with the respective rapi-

dity of the contractions which they accompanied, that of the auricles being slow compared with that of the ventricles, which was instantaneous and forcible.

2. With regard to *Dr Corrigan's* inquiry into the causes of the heart's impulse and sounds, we have seen that he denies the Hunterian doctrine, as to the cause of the impulse, ascribes this impulse to the auricular contraction producing dilatation of the ventricles, and imputes the second sound of the heart to the sudden and violent approximation of the walls of the ventricles, at the close of their contraction. Hunter ascribed the impulse to the tendency of the blood, while rushing through the arch of the aorta, to straighten the curve. But Dr Corrigan says that an impulse given to a fluid in a curved elastic tube has no such effect,—that it is a principle in hydraulics that the curve is either straightened outwards or curved more inwards, according as the orifice from which the fluid rushes is directed outwards or inwards,—and that he has ascertained this fact experimentally by fixing an elastic tube on the end of a syringe, and injecting water through it, so as to imitate the passage of blood through the arch of the aorta. Independently, however, of this consideration, he maintains he has proved, as we have already mentioned, that the impulse of the heart really precedes the impulse given to the blood in the aorta by the contraction of the left ventricle; and consequently, cannot be in any manner connected with the passage of the aortal blood. On the contrary, the impulse constantly attends the dilatation of the ventricles under the auricular contraction, and must therefore be caused by it.—Again, the first or dull sound of the heart, as it accompanies the impulse, must be owing to the passage of the blood into the ventricles from the auricles.—Lastly, the second or sharp sound is owing to the sudden approximation of the ventricular parietes; for it occurs, in point of time, exactly at the close of the ventricular contraction, and may be imitated in the dead body by tying the pulmonary veins, fastening an elastic tube into the aorta, filling the left ventricle with water, and suddenly exhausting the ventricle with a syringe.

We have thus endeavoured to do the author justice by stating his views and arguments in detail. In consideration of their great ingenuity and plausibility, we should be well pleased if we could conscientiously say that he has convinced us of their accuracy. But unhappily we are compelled to add, that from first to last his facts appear to us false, his arguments unsubstantial, and his conclusions erroneous. This heavy charge has been so fully proved against him by several able articles published lately in the London Medical Gazette, by *Dr James*

Hope, a recent graduate of Edinburgh, that, in commenting upon *Dr Corrigan's* supposed discoveries, it is unnecessary for us to do more than to analyze shortly his adversary's refutation.

Dr Hope sets out with denying the want of synchronism, which *Dr Corrigan* believes to exist between the heart's impulse and the radial pulse. This is the only material statement in his refutation which, as will presently be seen, is incorrect; but the error is not of much consequence to the rest of his commentary; for the pulse *at the root of the neck* is certainly synchronous with the heart's impulse, and the heart's impulse therefore presumptively synchronous with the ventricular contractions. Accordingly, when our author observed the rhythm of the heart's action,—not like Dr Corrigan, in a small animal, such as the rabbit, whose rapid pulse, approaching to 200 per minute, must, one would think, render it impossible for any man to follow the several movements of its heart,—but in a large animal,—in an ass, he and various gentlemen who witnessed his experiments, invariably observed the order of arrangement to be as follows:—1. Auricular contraction, not slow and heavy, as *Dr Corrigan* represents, but quick, short, and unaccompanied with either sound or impulse; 2. Ventricular contraction, which follows the contraction of the auricles with scarcely an appreciable interval, and is not sudden and rapid, as *Dr Corrigan* states, but, compared with that of the auricles, slow and long. At the same instant, the point of the heart is raised forcibly up, so as to produce the impulse, and at the same instant too is heard the first dull sound of the heart. 3. An interval of repose, during which the flaccid ventricles are rapidly filled with blood *before the auricles begin to contract.*

The first sound and the impulse, therefore, accompany the ventricular contraction, as Laennec and other physiologists believe. The cause of the impulse is, according to *Dr Hope*, some peculiar arrangement in the contraction of the muscular fibres of the ventricles, in consequence of which the apex is raised against the side; and the cause of the sound is the rushing of the blood from the ventricles. The second sound accompanies the dilatation of the ventricles; and its cause is probably the rushing of the blood into them from the auricles before the latter begin to contract. At all events, the ventricles do not produce this sound by the sudden and violent approximation of their parietes, as *Dr Corrigan* thinks; for the contraction of the ventricles comes to an end before the sound is heard,—their parietes really do not come in contact with one another, but are always separated by a little blood, even at the very end of their contraction,—and the apparent imitation of the sound procured by *Dr Corrigan* in the dead heart in the manner described above is not owing to the approximation of the parietes of the ventricles, which cannot be exhausted by the means he employed, but simply to the valves being suddenly applied on the open end of the exhausting tube. To conclude, although *Hunter* was mistaken in his explanation of the cause of the heart's impulse, *Dr Corrigan* is equally mistaken in the objection he deduces against it from the principles of hydraulics; for when a flexible tube, the elasticity of which tends to keep it bent, is fastened on the end of a syringe, and water injected by successive jerks, the tube is straightened considerably outwards at every descent of the piston, whatever be the direction of the open end of the tube.

We recommend these observations and experiments of *Dr Hope* to the attentive perusal of every student and practitioner, because they appear to give the clearest view yet taken of the rhythm of the heart's motions, and they are indispensable for understanding thoroughly the diagnosis of the diseases of that organ. But we may also recommend *Dr Hope* to reconsider what he says of the synchronism between the heart's impulse and the radial pulse. For here he is undoubtedly mistaken. If the fore-finger of one hand be held lightly on the apex of the heart, and that of the other on the radial artery, or still better on the posterior tibial behind the inner ankle, the observer will readily remark a sensible interval between the former and the latter;—the pulse follows the heart. *Dr Corrigan* is correct in making this statement. But he is wrong when he supposes he is the first who has noticed it; for the late *Dr Monro* used to mention the fact in his lectures *Dr Corrigan* is still more mistaken, however, in his explanation of the cause of the interval,—namely, that the impulse of the heart is owing to its auricular, and that of the pulse to the ventricular contraction. The contraction of the ventricles causes both, as *Dr Hope* shows; and the cause of the interval is simply that some little time is required for the transmission of the impulse along a fluid contained in an elastic tube. Both *Dr Corrigan* and *Dr Hope* seem to think, that, according to the principles of hydraulics, the impulse ought to be felt instantaneously at the commencement and at the most distant end of the arterial tubes, because they are constantly full of blood. But this is not a correct view of the matter. The arterial tube being elastic, the impulse is communicated somewhat after the manner of a wave, and not instantaneously. The hose of a fire-engine in action is somewhat similarly circumstanced with the arteries; and there the propagation of the impulse along the tube may be followed with the eye. And in fact, if the finger of one hand be applied to an artery close to the heart, such as the supra-scapular at the root of the neck, and the finger of the other hand to the posterior tibial behind the inner ankle, the same interval will be observed as when the former finger is applied to the apex of the heart; and if the fingers be applied, one over the heart, the other over the supra-scapular, the two impulses are felt to be perfectly synchronous.

We may follow up this analysis of the elaborate paper of Dr Corrigan with a short notice of two other papers on correlative subjects,—the one an account of a case of complicated disease of the heart, the other a history of two cases of supposed polypus of the heart.

Dr Clinton's case of diseased heart is a full and circumstantial description of an instance of enormous enlargement of the left and slight enlargement of the right auricle, together with considerable contraction of the tricuspid, extreme contraction of the mitral, and also great contraction of the aortal valves. The left auricle could hold nearly the fist. The ventricles were healthy. The ventricular sound, or that which accompanies the impulse of the heart, was alone attended with the *bruit de soufflet.*

Dr Harty's cases of polypus of the heart are detailed minutely by the author with the view of proving, that such a disease really occurs sometimes, although some eminent pathologists entertain doubts on the subject,—and that it may be known by a characteristic or pathognomic symptom. In both his patients, who were about or under the age of puberty, there was, besides the ordinary symptoms of diseased heart, a marked preference of the complete horizontal posture, with great increase of dyspnœa and palpitation whenever the sitting posture was assumed; but the symptom which Dr Harty considers pathognomic was a vibrating pulse, "communicating a peculiar thrilling, whizzing sensation to the finger." In one of the cases the organic derangement consisted in a longitudinal division of the left ventricle by a membrane "firmly adhering to the apex and to the sides of the ventricle in a line nearly parallel to the septum, and terminating, as it approached the aorta, in a rounded organized polypus, tapering to a point, and entering about an inch into the aorta." In the other case the polypus appeared to take its origin in the left auricle, with the inside of which it "maintained a perfect union by means of a number of lateral projections;" and passing through the auriculo-ventricular opening it contracted equally firm adhesion to the ventricle. At the same time it served as a point of attachment for the ordinary pseudo-polypi, which were extensively ramified into the aorta and pulmonary veins.

The next papers we shall notice are two contributed by

Dr John Ferguson.—One of these contains a good account of two cases of pulmonary apoplexy, the one fatal, the other a case of recovery. The stethoscopic signs of the disease are well delineated, and an excellent account given of the morbid appearances in the fatal case. These were the complete injection of the upper lobe of the left lung with blood, laceration of the pleura on the posterior surface of that lobe, and extravasation into the pleural sac of about three quarts of blood. This is a very rare termination of pulmonary apoplexy. The patient, as will be easily understood, died very suddenly. After labouring for some time under cough, trifling hæmoptysis, and general oppression of the chest, he one morning, while in the act of putting on his shoes, complained of loss of sight, fainted away, and died on the spot.—The author's second paper, on the application of the stethoscope to determine the existence of pregnancy in doubtful cases, is of much greater interest. It is one of the first attempts publicly made in Britain, if not the very first, to recommend the use of the stethoscope for such a purpose. A few years ago *M. Kergaradec* discovered, that after the fifth month of pregnancy a delicate stethoscopic ear could detect the pulsation of the fœtal heart in one of the iliac regions at least, if not also even over the whole abdomen, while in the opposite iliac region a sound like a feeble "*bruit de soufflet*" was likewise to be felt, arising, as Dr Ferguson thinks, from the passage of the blood in the arteries connecting the placenta with the uterus. British accoucheurs appear to have paid little or no attention to this alleged discovery; for with the exception of our author's researches, and the apparently contemporaneous observations of Dr Kennedy, which will be noticed in our review of the Dublin Hospital Reports, we are not aware that any one has made it the subject of trial. Yet, according to Dr Ferguson, it is an infallible criterion of pregnancy, and of course, if really such, it is also the only infallible criterion of that state. He says he has never in a single instance been mistaken when he announced the existence of pregnancy, and in one instance only out of very many trials was unable to detect the stethoscopic signs in undoubted pregnancy. Of several very interesting examples of concealment of pregnancy which are related to illustrate the practical value of this method of diagnosis, we may select the following singular history.

"In November 1827, a young woman, aged 22, applied at the Dublin General Dispensary for relief. She gave such an account of herself, as induced me to think she laboured under an aggravated form of dyspepsia. She told me her menses were regular, and that her abdomen, which I observed to be enlarged, was so only occasionally; indeed, such was the excellent arrangement of dyspeptic symptoms which she stated herself to labour under, that she completely blindfolded me. However, on her third visit to the Dispensary, some incoherence in the account of herself, and, as will be readily imagined, no beneficial effects following the exhibition of the medicines ordered for her, excited my suspicion; when, as was my habit with pregnant women, I employed the stethoscope. The riddle was soon read. I almost immediately detected the heart of a fœtus, and also the placenta; nor, which is a matter of some moment in such cases, had the patient the slightest suspicion of the object of such an investigation. She received the announcement of her being pregnant with extreme indignation; indeed, this young lady's histrionic talent was of the first order; and such was her well-feigned agony at the very idea of her virgin innocence being even suspected, that, had I not the positive evidence of my senses to confirm me in the opinion I had expressed, I should have felt extremely uncomfortable in the situation in which I found myself. I took some trouble to convince her of the impossibility of my being mistaken. Having obtained from her the address of her friends, despite her most urgent entreaties to defer the disclosure for some time, as, by her own acknowledgment, she was only in her fifth month, I communicated the fact to them, with all the palliation I could adduce, and with a good deal of difficulty quieted the storm of angry feelings, which such an announcement was well calculated to elicit. They forgave her, however, and promised to receive her."—P. 72.—An accouchement ensued in due time.

The following instructions and precautions are laid down by Dr Ferguson, as necessary to the correct use of the instrument.

" The placentary noise should be sought for in either iliac region, where, at least according to my experience, it will be most generally found. Yet have I detected it in almost every part of the abdomen; nor does it ever vary from the place where first heard.

" The only error that I am aware of, into which we are liable to fall in making this examination, is, where the pulsations of the iliac arteries are accompanied with a *bruit de soufflet*. This, of course, is well calculated to deceive; but when the artery is the source of the *bruit*, we shall hear it, I am inclined to think, equally on both sides, at least I never met a case where this did not hold. Besides, it will not be heard save in the groin; whereas the noise of the placenta is heard over a space of some extent, perhaps three or four inches square.

" The fœtal heart I have heard in almost every region of the abdomen. Although it and the placenta are sometimes heard in the same side, and I have even found them in the same spot, yet have I observed that they were to be met with in the majority of cases, at opposite sides, generally in the iliac region; so that this is the situation which I always examine first, and have rarely occasion to shift the instrument to any extent before either one sound or the other is discovered. The fœtal heart, however, unlike the placenta, is not always heard in the same place in the same individual; that is, it may be found to-day in one point, and in another to-morrow; though I have never found it to vary much from the point where first heard. Its double beat is well-marked; and the frequency of its pulsations is, I believe, always much greater, often double that of the mother's."—P. 69.

The utility of this method of investigation can only be judged of after long and frequent trial by practised stethoscopists. In the meantime, it obviously deserves an attentive examination.

We shall pass next to the papers of *Dr Crampton*, which are two in number. One of them is an account of a good case of melanosis in the person of a weaver, thirty-four years of age. It was considered during the patient's lifetime, as an instance of enlarged and tuberculated liver. After death melanotic tubercles were found on the pleura; but the principal melanotic deposition had taken place in the liver, which weighed nineteen pounds, and was thickly studded with black tubercles, varying in size from that of a plumb to that of a middle-sized apple.—The second paper by the same author, is a history of a case of perforation of the stomach, attended with the passage of a lumbricus through the aperture. The patient, an elderly female, was admitted as a fever-patient into the Hardwicke Fever Hospital. She had obstinate constipation, vomiting, epigastric pain, and tympany of some days' standing; afterwards she had diarrhœa without vomiting; and she died a fortnight after admission, much emaciated. The whole peritonæum of the viscera was covered with lymph; there was also lymph on the pericardium and heart; and a puriform fluid was effused copiously into the abdominal cavity. The lumbricus lay between the stomach and concave surface of the liver. The stomach, at the middle of its lesser curvature, was perforated by a hole with a defined, triangular, hard, elevated margin, on which a little ulcer had formed. The hole exactly admitted the worm; and the stomach elsewhere was healthy. There were no more worms in the intestines.

Dr Robert Law contributes two papers, one on putrefactive degeneration of the lungs,—the other on hæmatemesis dependent on diseased liver. In the first of these he endeavours, by a perhaps unnecessary refinement, to distinguish between gangrene of the lungs, and sloughing of these organs. The latter he considers analogous in its origin and circumstances to the sloughing caused on the external parts of the body by diffuse cellular inflammation. There is a want of the usual tendency to circumscribe the inflammation of the effusion of lymph; vessels are in consequence laid open, giving rise to profuse hæmoptysis; portions of the lungs are isolated, and putrefy, giving rise to fetid breath and sputa; and the formation of large cavities which communicate freely with open bronchial tubes, and sometimes with open vessels; and occasionally when these changes occur near the surface of the lungs, the pleura gives way, so that pneumothorax and empyema arise. Four excellent cases are given as examples of the ordinary course of the disease. The pathognomic signs were always putrid breath, frequent copious hæmoptysis and much temporary relief after the sudden discharge of putrid pus or blood.

In the second paper he calls the attention of practitioners to the frequent occurrence of hæmatemesis in some cases of diseased liver, of a kind not easily recognizable by the usual symptoms. The disease with which he has found it to concur appears to be the advanced stage of that variety of tuberculated liver that attacks drunkards, and is thus described by him.

> " This diseased condition of the liver would appear to consist in an entire change of the proper substance of the organ, into a dense fibro-cellular tissue, forming a kind of pulp or parenchyma, in which the roundish bodies (which, projecting on the surface, give the organ its irregular appearance,) are imbedded, its specific weight is greater than natural, while its actual size is less, and, so far from passing the margin of the ribs, it seems to have receded within its ordinary bounds, showing how fallible a criterion of the health of the organ is either its size or descent below the ribs; its form is altered; it becomes more round; its anterior margin more obtuse; its division into lobes less defined; its peritonæal covering thicker and more opaque than natural; its section exhibits no trace of blood-vessels, which we may presume to be either compressed or obliterated by the altered structure. Hence the impediment to the vena portæ pouring its blood into its ordinary channels, and the influence of the obstruction reverts upon all the branches which concur to form this vein, and thus we do satisfactorily account for the hemorrhage."—P. 108.

The only other papers which require notice, are several short obstetric essays by *Dr Collins*, *Beatty*, and *Montgomery*.

Dr Collins contributes two cases of recovery from ruptured uterus, and a case of extra-uterine pregnancy. In one of the former, an extensive laceration of the vagina and cervix uteri was produced by a midwife mistaking the hand for the foot, and attempting to extract the child when the shoulder presented. On the day after delivery there was considerable tenderness in the hypogastrium, which was removed by thirty-six leeches and constant fomentations; the woman never had a bad symptom afterwards; and in twenty-three days she left the hospital quite well. The cause of the laceration in the second case was not very obvious. In the midst of natural labour, the uterine action suddenly ceased, and great debility, vomiting, and anxiety of countenance ensued after delivery. There was found at the junction of the vagina and cervix uteri an extensive laceration, through which the intestines portruded. This patient had threatenings of uterine and peritonæal inflammation for several day; but the symptoms were kept under by the frequent use of leeches and constant fomentation; and the woman eventually recovered. After the first fortnight a large quantity of purulent matter was twice discharged from the vagina. In the case of extra-uterine pregnancy, death took place about two months after impregnation. On dissection the pelvis was found full of blood, in which was enveloped a fœtus about the second month. The hemorrhage had proceeded from the rupture of a tumour in the pelvis, which had contained the fœtus, but which had not the slightest connexion with the uterus or its appendages.

Dr Beatty's contributions consist of a case of cancer of the uterus, and some remarks on the safety of the delivering forceps in tedious labour. The only particular worthy of note in his case of uterine cancer is, that a communication had taken place between not only the rectum and vagina, but likewise the vagina and ilium; in consequence of which, the patient was exempt from the obstinate constipation which usually attends the advanced stage of this disease. The author thinks he has traced uterine cancer very frequently to the sudden abandonment of sexual intercourse in those much accustomed to it, and hence accounts for its frequency among widows.—*Dr Beatty's* second communication is a defence of the early use of the forceps in cases of tedious labour. This is grounded chiefly on his own great success. He states that during forty-two years, in which he has been actively employed in the practice of midwifery, he has delivered in his private practice one hundred and eleven women with the forceps or lever; and that "in no instance did any unpleasant result follow. None of the mothers died, none of them had the perinæum lacerated, nor any of those evils which are set forth as the effects of the forceps; and still more, all the children that we had any reason to think were alive at the commencement were born living, and none of the whole number had any injury or mark whatever inflicted by the instrument."

The communications of *Dr Montgomery* are a case of ovarian tumour, and another of malformation of the fœtus,—both of a rare and rather singular description. The leading circumstance in his narrative of the case of ovarian enlargement is the nature of the tumour, which we shall describe in his own words.

"On turning aside the (divided) integuments, a very singular appearance presented itself; a tumour, chiefly composed of fine membranes, dividing it into innumerable cells, which, with their fluid and transparent contents, resembled at first sight hydatids. The membranous septa dividing the cells were supplied with blood-vessels of a considerable size running along their edges, so that the whole tumour presented a clear red colour. At its upper and left part there was a deep cleft or fissure, into which the open hand might be passed without any force, and when carried downwards and towards the right side, it entered a round sac equal in size, and much resembling a large flat turnip; this was the right ovary, which lay just under, and was filled with the same structure as the part of the tumour first brought into view.

"In fact it seemed as if the peculiar structure had first grown in the ovary, which thereby became greatly enlarged, until at length the coat of the ovary had given way, and out of the fissure so formed, the morbid growth continued to enlarge, turning over the edges of the fissure, and covering the front and sides of the ovary in which it had formerly been contained, so that the tumour was in a great measure turned inside out."—P. 138.

The tumour was twelve inches long and nine broad, and reached as high as the ensiform cartilage.

Dr Beatty's case of malformation of the fœtus is remarkable in many respects; and perhaps not the least striking circumstance regarding it is, that no such monster has been described even among the *Monstruosités Humaines* of Geoffroy St-Hilaire. In addition to other less extraordinary deviations from the common structure, the whole alimentary canal from the cardia downwards, together with the liver, spleen, and pancreas, were situated *on the outside of the integuments of the abdomen*, which lay in contact with the spine almost throughout; the great intestines, from the appendix vermiformis downwards, were wanting; and the external and internal organs were malconformed in a very complicated manner, which we cannot make intelligible without a longer description than is here admissible.

Dr Thomas Ferguson's case of anomalous labour, and Mr Purdon's case of hydrophobia, do not appear to possess particular interest.

The Medical Report of Dr O'Brien, which we have not yet noticed, is the most elaborate and extended article in the volume. But as it has been published in a separate form, we have considered it better to make it the subject of a distinct review.

ART. II.—*Medical Report of the House of Recovery and Fever Hospital, Cork Street, Dublin, for the year* 1829. By JOHN O'BRIEN, M.D., Vice-President of the King's and Queen's College of Physicians in Ireland, &c. &c. &c. Dublin, 1830.

THOUGH much attention still continues to be given to the subject of fever, it is by no means yet exhausted; and the appearance of an authentic report from one of the greatest fever hospitals in these countries, must excite considerable interest, particularly as the author of this report is a gentleman well known from his writings on various subjects connected with practical medicine. Dr O'Brien is manifestly an observer of disease, and a faithful reporter of many of its varied phenomena, and deserves great credit for the zeal with which he has prosecuted his researches. But whether he has done any thing original in the field connected with his present subject, we shall proceed to inquire.

It appears from the table affixed to the report, that from the year 1804 to 1830, the number of 81,418 patients has been admitted into the Cork Street Fever Hospital. Yet we regret to remark that, notwithstanding this extensive opportunity, but little, if any, information of practical utility has proceeded from this institution. Annual reports indeed have been published, but they contain chiefly an account of the prevalence of fever during the past year, and the character it presented in hospital, but we hear of no dissections, although, by the table before alluded to, the deaths have amounted to 5085 during the above period. This unequalled opportunity for pathological research has been lost to posterity, not by any neglect or remissness of the physicians of the institution, but by an absurd regulation of the hospital, which prohibits dissection there;—a regulation well adapted to prevent the medical officers from improving their knowledge and treatment of the very disease that the institution is intended to prevent or relieve.

Dr O'Brien commences with some meteorological observations, which do not possess particular interest. In the next division of the work, however, on the statistics of fever, we find the following important remark on the etiology of the disease.

"There is one fact, however, which the author thinks it important to notice, and which he conceives to be a decisive refutation of that opinion which would attribute the generation of epidemic fever to the sole and exclusive agency of distress and famine; namely, that during the period of epidemic intermission, (if we may be allowed the phrase,) just now described, when this disease had nearly disappeared from among us, public distress and suffering had never previously, perhaps, attained to such a height in Dublin."—P. 6.

Dr O'Brien does not, however, deny the influence of want and misery in the production of fever; he looks upon them as the predisposing causes in this country, but holds the opinion that these evils alone are incapable of generating continued fever in any individual instance, much less in its epidemic form. To produce this effect, the agency of another set of causes appears to him indispensably necessary,—an opinion with which we cannot help coinciding, proved as it is by the observation of the past year, when the city of Dublin enjoyed an extraordinary immunity from fever, although public distress subsisted to an extreme degree.

In commenting on the mortality of the hospital, the author shows, that when the admissions were most numerous the mortality was least.

> "Thus in the great epidemics which occurred in Dublin in the course of the last twenty years, the mortality in 1815, was 1 in 20; in 1818, 1 in 30; and in 1826, 1 in 28; while in the three years 1823, 1824, 1825, which present a considerable reduction in the admissions, the total mortality was 1 in 11; and in the last two years, 1828, 1829, the total mortality was 1 in 14. The inference from this fact is, that the majority of cases which constitute our epidemic fevers are of a mild character."—P. 9.

In these conclusions we believe that there is some error. It is notorious, that during the pressure of a severe epidemic the average time allowed to each patient in hospital is greatly diminished; vast numbers are sent out at a very early period of their convalescence, exposed to all the dangers resulting from relapse, or the various sequelæ of fever, dangers which, under usual circumstances, they would have gone through before leaving the hospital. It is obvious that, under these circumstances, the apparent mortality must be slight.

Passing over a sketch of the different theories of fever, and some cases detailed at length, we come to the author's pathology of fever, deduced, as he states, from the symptoms. Here, however, we must protest against his opinion, that dissection, as compared with the study of symptoms, is of little consequence. "*Qu'est l'observation si on ignore ou siege le mal,*" said Bichat; but Dr O'Brien argues against the utility of dissection in throwing light on fever, because it *is an acute disease of a protracted character*, in which new disorganizations and decompositions are perpetually taking place through its whole progress. Morbid anatomy, he thinks, may discover the causes of death, but is incapable of developing the first cause of disease. Now, it appears to us that this argument will apply against dissection in diseases of all descriptions, whether they be simply acute, or, to use his own words, acute, but of a protracted character,—an expression which is not very intelligible, and which at least implies some contradiction.

Dr O'Brien admits the existence of a primary *gastro-enteritis*, attended by a fever of a peculiar kind, approximating in some respects to the typhoid character. He further maintains, that this may supervene on typhoid fever, and modify its character. Lastly, He contends, that idiopathic continued fever does not exist, of which two varieties are to be enumerated; the one inflammatory (synocha,) in which the morbific cause exercises its agency principally over the heart and arteries, while the sensorium and nervous system enjoy comparative exemption from its influence; the other typhoid, including Cullen's synochus and typhus, where the operation of the morbific cause is previously and essentially directed against the nervous system, and through that system against the heart and arteries and their capillary extremities.

Such is Dr O'Brien's theory of fever as deduced from his own observation. It certainly has the merit of being very comprehensive, embracing as it does all the modern doctrines on the subject. In the present state of medicine, we cannot have a better; but it must be admitted as a proof of the difficulty of framing a theory of this malady, that seventeen years of experience have not enabled a physician like Dr O'Brien to form something more definite. The first part of the doctrine corresponds with that of Broussais, Pinel, and Rœderer and Wagler; the second is that of Tweedie, and partly of Smith; the third is a modification of the Cullenian system.

We are by no means satisfied that the disease termed by Dr O'Brien primary gastro-enteritis is not an example of continued fever, upon which gastric irritation has supervened, modifying the original character of the disease, as noticed by Dr Tweedie. The following is Dr O'Brien's description of this disease.

" Pain, uneasiness, and generally fulness of the epigastrium or abdomen, or both; aggravated by pressure, and accompanied by headach, nausea or vomiting, particularly after the introduction of the smallest quantity of fluid or solid aliment into the stomach. The appearance of the tongue is peculiar and characteristic; it is either of a vivid or a dark-red colour over its entire surface, or it is red at the edges and point, butcove red with a dark-white fur in the centre, through which specks of red are occasionally visible. The centre, however, is frequently brown, or even of a yellowish hue, whilst the edges are dark-red, as above described, and the papillæ all over the surface are unusually pominent, and this organ, on the whole, presents a more striking appearance of irritation and sub-inflammation in this disease than in any other type of fever. The pulse is usually deficient in fulness, it is small and compressible, and approximates more to the typhoid than the synochoid character. It is also accompanied by a lower temperature of the skin, and, in a word, displays none of the signs of reaction which mark the early stage of synochus. It is distinguished, however, from typhus by the comparative mildness of the cerebral affection. The author has indeed been frequently surprized at the clearness and integrity of the intellectual faculties in the midst of that extreme depression of the muscular power which characterizes this type of fever."—P. 51.

Dr O'Brien states farther, that this disease is to be distinguished from typhus by its slow progress, the absence of petechiæ, and of sordes.

We submit, however, that there is nothing in this picture of disease to induce us to believe that the affection differs at all from the common gastric fever of these countries. We have seen the above symptoms again and again occurring during epidemics of fever, clearly traceable in many instances to contagion, and succeeded by *petechiæ*, *sordes* on the teeth, *subsultus*, muttering delirium, and all the symptoms of the last stage of typhoid fever. Dr O'Brien has brought forward no evidence to show that these symptoms were not secondary to that state termed fever, according to the views of Dr Tweedie. He has failed to make out his case. The integrity of the mental faculties is much relied on by him, as if nervous excitement was never symptomatic in simple gastro-intestinal inflammation. The truth is, we may have typhous fever with integrity of the mental faculties, and, on the other hand, primary irritation of the digestive tube, either acute or chronic, with all the varieties of delirium. The absence of delirium, then, does not prove that the disease is not an example of typhous fever.

The same observations apply to the occurrence of petechiæ and sordes. The author, as a further proof of the nature of this disease, states, that, in proportion as the abdominal symptoms were mitigated or removed, the affection of the head and the febrile symptoms suffered a simultaneous mitigation or removal. Surely this does not prove that the gastro-enteritis was primary. Daily experience shows us that, in a case of fever where an important organ becomes secondarily engaged, relief will follow the removal or mitigation of that state.

We now come to the best part of the work, namely, the author's treatment of fever. That Dr O'Brien must be a successful practitioner in fever, we can have little doubt, as in his practice he is not carried away by any overstrained theory. He even admits with candour that his own views do not furnish a principle which he could at once apply to the treatment of fever in all cases, and taking experience as his chief guide, he proposes a plan of treatment, which, though containing nothing original, yet, taken as a whole, is highly deserving of attention. Discarding the idea, that fever can be cut short by any stroke of art, he looks upon the disease as one which must go through its course, and directs his attention chiefly to the local inflammations which occur, and which constitute the principal danger of the fevers in the present time. Speaking of the form of fever termed by him "*primary gastro-enterite*," when the fever assumes the synochoid character, Dr O'Brien commences his treatment, "By taking twelve or fourteen ounces of blood from the arm,—a measure which will also be generally adviseable when the pain of the epigastrium and abdomen are very acute, and increased by slight external pressure. The remainder of the treatment may be conducted by the repeated application of leeches to the epigastrium or abdomen, according to the nature of the case, until all pain and tension be removed. As the number of leeches at each application may be considered a point of importance, it may be stated, that the maximum number in the four first cases recorded above, and which were cases of great severity, was twelve at each application, and even from this number (which some will probably consider too small) the author has invariably found a decided benefit. After the leeches, it is usual to apply warm stupes, or an emollient poultice over the part most affected. Next in efficacy to local blood-letting we would place laxatives; irritating purgatives are not applicable here, though we by no means sympathize in the horror which the disciples of M. Broussais entertain against this class of medicines."—Page 61.

Dr O'Brien speaks highly of the use of calomel and opium in this disease, and has found it to allay the irritation of the stomach when all other means failed; but we cannot agree both in thinking that salivation should be produced in this disease. We have seen some very unpleasant consequences follow this mode of treatment, such as an exasperation of the gastro-intestinal irritation, and often a most troublesome, and in several cases dangerous, ulceration of the gums and lips. Dr O'Brien looks on moderate diarrhœa as a sanative process in the inflammations of the gastro-intestinal system.

"In such cases our object is to moderate, not suppress this discharge, and for this purpose we have found Dover's powders and rhubarb one of the best remedies, in conjunction with gummy and mucilaginous diluents. Secondary inflammation of the mucous or serous structures of the lungs are by no means an unfrequent complication in this disease. Such cases are to be treated according to the ordinary method by general blood-letting, if the strength of the patient permit, but if not, by the local abstraction of blood, blisters, and expectorants."—Page 62.

In the treatment of the secondary gastro-enteritis which supervenes on typhoid fever, Dr O'Brien prohibits general blood-letting altogether, and opposes the disease by the repeated application of leeches, the use of laxatives and mucilaginous medicines. This mode of treatment, first proposed by Beddoes, we have ourselves seen singularly successful, and regret that it is not yet extensively adopted in these countries. Whether the gastro-intestinal irritation be secondary or primary, its removal is the great object, and on this depends in a great measure the success of our treatment of gastric fever. The author's treatment of typhoid fever is equally judicious;—palliative in the first instance, tonic and moderately stimulant in the latter stages. He never uses general bleeding, unless the reaction in the early stages be severe, and then employs it with caution. Wine he exhibits reluctantly, and never until the latter stages of the disease.

In a short notice on intermittent fever, the following statement deserves attention.

> " A singular revolution has taken place in Dublin for the last two years, with respect to intermittent and continued fevers. The latter disease, which for a century has been the constant scourge of this city, has considerably declined, and intermittent fever has sprung up and occupied its place. For the last four months, however, up to February 1830, intermittent fever has again disappeared, and, I am happy to say, without a corresponding increase of continued fever. These facts do not easily harmonise with the established doctrine of the etiology of intermittent fever. We have no reason to conclude that a more abundant extrication of *malaria* has occurred in the last two years than previously. Of late years this city has undergone a signal improvement, both as to the arrangement of its streets, its cleanliness, and in an abundant supply of fresh water in all its parts."—Page 73.

Dr O'Brien speaks of the practice of bleeding in the different stages of ague, but allows that the cases in which this operation was performed were too few to enable him to draw certain conclusions. He is disposed, hewever, to think, that blood-letting, performed at any stage of the fever, tends to shorten the duration of the paroxysm and that of the whole disease.

After concluding his observations on fever, the author devotes a section of his report to the subject of inflammatory diseases of the lungs This is opened by a formal attack on the practice of mediate auscultation.

> " With respect to the stethoscope, highly ingenious as the application and discovery of this instrument certainly are, yet we fear that Laennec attached too much importance to it as a means of diagnosis, and that in labouring to elevate his favourite discovery to the dignity of a science, he has occasionally deviated into the regions of fancy, and been indebted to the fugitive creations of that active faculty, for *some at least*, of those acoustic phenomena which he represents as permanent and infallible diagnostic signs.
>
> " From the works of an eminent and able living writer of the present day, *(Dr Boisseau,)* whose writings prove him to be perfectly qualified to pronounce an opinion on the subject, and whose evidence will hardly be accused of prejudice on a question where

national feeling is apt to interfere in behalf of national discovery, the author extracts the following remarks on the stethoscope.

" 'When Laennec first exhibited the stethoscope, people expected to be able to read as it were by this instrument what passed in the chest. A little further observation, however, has taught us that its signs are obscure, uncertain, and not to be depended on without the symptoms. In phthisis this instrument is incapable of discovering tubercles until they become hollow cavities, when their discovery is of little use. The pathognomonic stethoscopic signs of phthisis are the cavernous voice and *souffle à l'oreille*, which connot he heard until the cavity of the tubercle has acquired a considerable magnitude, that is, in fact, until the tubercle has ceased to exist.'—*Nosograph Organ.*

" We have been led into the above remarks by the desire of repressing unreasonable enthusiasm on the one hand, and, on the other, that disingenious system of puffing by which some persons affect to, if not to read, at least to hear more than their neighbours by this instrument."—Pp. 76, 77.

These remarks we read with regret; and to allow them to pass unanswered, would be to fail in our duty to the public. The charge against Laennec, of being indebted to his fancy for many of the acoustic phenomena which he so accurately describes, may be easily shown to be wholly groundless. It is now fourteen years since the practice of auscultation was applied to diseases of the lungs, since which time the use of the stethoscope has been practised in all parts of the world. New phenomena have been observed, the principles of diagnosis have been confirmed, and, in some instances, they have been modified. But not a single sign of disease, as given by Laennec, has been proved to be false. The names of Soemmering, Andral, Louis, Reynaud, Bertin, even Broussais, besides a host of minor authorities, may be quoted among the continental physicians who have verified his statements. In Great Britain, we have those of Duncan, Elliotson, Forbes, William Cullen, and Gregory, and in Ireland, Graves, Stokes, and Townsend. From these gentlemen, numerous works and essays have proceeded, all confirming the statements of the author of auscultation.

Dr O'Brien must have but little experience in the stethoscopic examination of phthisical patients, when he admits with Boisseau, that the existence of tubercles in the lungs cannot be discovered by the stethoscope till cavities are formed. Tubercle may be detected in a very early stage by the diminished intensity of respiration in the upper portion of the lung, as compared with the lower;—by the comparison of the respiratory murmur in the upper portions of both lungs;—by various modifications of *rale*, particularly the large *crepitus*, occurring under the clavicles, or in the supra-spinous regions. This we have verified repeatedly, and have traced the disease from the time when it produced only a slight diminution of the respiratory murmur to the formation of a distinct cavity. We do not deny that, in certain cases of phthisis, it is difficult to detect the formation of tubercle in its early stages; but we contend, that, in most cases, an accurate comparative observation of both lungs will detect the incipient disease. None of the authorities we have quoted ever maintained that stethoscopic observation was to supersede the study of symptoms; but they all admit that we must combine both these lights to arrive at an accurate diagnosis.

When an hospital physician makes a serious attack on an established mode of diagnosis, we expect at least that he has studied the subject carefully. This, however, we regret to state, does not appear to have been done by Dr O'Brien. Take, for example, the following specimen of accuracy in stethoscopic observation.

> " Patrick Norton, æt. 50, Jan. 6, states, that he was attacked by his present illness about three weeks ago, when he got cold by working in a damp cellar. He was at first affected with severe cough and spitting, which gradually grew worse; his face is swollen and purple; his hands and feet are also purple; respiration short, hurried, and laborious, with an incessant short cough; pulse nearly natural as to frequency; had weak tongue, loaded with a dark-white fur. On applying the stethoscope, *respiration was lost in all the lower part of the chest, and audible only at the upper part of the stomach.* Intellect clear. He is not conscious of his approaching death. Died in thirty-six hours after admission."—Page 85.

In this case the observation is not sufficiently precise to frame any judgment. There is no statement of the results of percussion, or of the occurrence of bronchial respiration, of egophony, of the effect of change of position on the stethoscopic phenomena, of dilatation of the side, and of other circumstances calculated to clear up the diagnosis between double pneumonia, empyema, or hydrothorax, not to speak of other diseases.

Speaking of acute *bronchitis*, Dr O'Brien says, " When the stethoscope is applied below the bifurcation of the *bronchi*, some of the numerous varieties of the *rale sonore*, so fancifully described by Laennec, are heard, or the respiration is inaudible over the inflamed spot. The crepitous *rale*, which Laennec describes as the peculiar and pathognomonic sign of peripneumony, is sometimes present in *bronchitis*, and is then supposed to indicate an œdematous state of the lungs."—Page 83.

In our author's account of pneumonia he details a case which appears to him decisive against auscultation.

"On applying the stethoscope a loud, sonorous, and mucous rattle are heard, which latter occasionally become crepitous, but these sounds mix and confound each other. On applying the tube over the seat of pain, I fancy I hear a hollow sound, and the voice appears to pass through the instrument."

In the course of seven days this patient was convalescent; an occurrence which elicits the following remark.

"Had I been governed by the stethoscopic signs in this case, I should have been much perplexed. The inference to be drawn from the first exploration was, that a cavity or abscess existed in the right lung, yet the rapid and perfect recovery of the patient showed that no such abscess could have existed."—Page 89.

This is a very imperfect piece of reasoning. Dr O'Brien makes a doubtful observation—from which he draws a certain conclusion, namely, that an abscess was pointed out by the stethoscope; but this he reverses, because the patient recovered, as if recovery from pneumonic abscess was a thing impossible. More acquaintance with the actual state of pathology would have saved him from this error; and Laennec would have told him that in the year 1823 eighteen cases out of twenty of pneumonic abscess terminated in perfect recovery.

Our author's praise of the stethoscope is equally unfortunate.

"In cases where an affection of the liver may be combined with that of the pleura, and doubts may arise, whether the pain of the side be primary or sympathetic, the stethoscope is of considerable value, as the presence or absence of respiration in the side will determine this point almost with certainty."—Page 90.

We have no hesitation in saying, that of all stethoscopic diagnostics, this is the most difficult to establish. An enlarged liver will produce absence of respiration even to a great extent—dulness of sound on percussion of the whole side—dilatation of the side. In a case of this kind, it is often next to impossible to say whether the pleura be involved or not—and we can assure Dr O'Brien, that the presence or absence of respiration comes to be of little or no value.

In concluding these observations, we only regret that Dr O'Brien should have been induced to condemn a mode of diagnosis, the merits of which he has not thoroughly studied.

Art. III.—*The Dublin Hospital Reports and Communications in Medicine and Surgery.* Volume the Fifth. Dublin, 1830. 8vo. Pp. 631.

AFTER an interval of unusual length, we are happy to see the fifth volume of these reports, which promises to sustain the reputation of its valuable predecessors.

The clinical report of the cases in the wards of the Meath Hospital, by Drs Graves and Stokes, which constitutes the first part of the volume, is the most ample and copious, and first deserves attention, both from this circumstance, and also from the importance of the facts which it contains.

The diseases considered in the Report are referred to the following four classes; those of the arterial system; painful swelling of the lower extremity, similar to *phlegmasia dolens;* diseases of the lymphatics; diseases of the organs of respiration, and diseases of the abdominal viscera. Were we in a censorious vein, we should remind the authors that this is not a philosophical division, and that logical arrangement requires a uniform principle, and not to make structure the basis of one class, function of another, and region of a third. Overlooking this, however, we proceed to inquire whether it is compensated by the materials.

Of the cases of arterial disease, the first is an instance of inflammation of the arteries of the right inferior extremity from the origin of the common iliac to the greater part of the anterior and posterior tibial arteries and the peroneal. The internal membrane of these vessels was red, soft, and covered by albuminous exudation; and the obstruction occasioned to the course of the blood caused the formation of a clot extending from the right common iliac downward to the tibial arteries. The effects of this obstruction to the free motion of the blood, were, with want of pulsation, extreme coldness of the limb to the extent of 38° below the natural standard, and want of sensation, followed by gangrenous inflammation of the skin and cellular tissue. The latter morbid action the authors regard as the effect of the effort to restore the collateral circulation, and, therefore, as indicating excessive, not deficient circulation in the limb, in opposition to Andral, who considers it as the nature of *gangræna senilis.* The difference we would consider more in name than reality, were it not made the foundation of a good practical rule, that warm applications are improper and injurious after the limb has begun to recover its temperature.

A case of aneurism of the abdominal aorta gives rise to some observations on the diagnosis of that disease. It is well known that aneurisms in their early stage are not unfrequently mistaken for rheumatic affections, or disease of some other organ than the arterial tube; and that aneurism of the abdominal aorta has been often mistaken for a disease of the kidney. Yet it must be regarded as a proof of the extreme difficulty of establishing an unequivocal diagnosis of this disease, that not one of the symptoms enumerated by the present authors can be depended on as certainly indicating the existence of the disease. The most common symptoms are doubtless the lumbar colicky pains;

but that of nausea is evidently not peculiar to abdominal aneurism, and is certainly, when present, a proof that the tumour has enlarged so much as to implicate the upper division of the abdominal aorta. This is proved by the fact, that, in the case reported by the present authors, the tumour was so large as to push the heart forwards and to the right side, so that its pulsations were felt first in the epigastrium, and afterwards in the right side of the sternum altogether. It is unfortunate, nevertheless, that inspection of the body was not permitted.

A case of thoracic aortic aneurism is interesting, chiefly by originating after mental emotion, by being unaccompanied with symptoms of cardiac disease, and by the tumour emitting a double pulsation.

To the second head of diseases, the reporters refer a painful swelling of the lower extremity, occurring in general in the course, or at the termination of fever, and sometimes, though not always, connected with pain in the course of the saphena vein, or with the interior of the knee-joint. Though the reporters observed this affection to occur indiscriminately in intermittent and continued fever, they entertain no doubt that it is pathologically the same as *phlegmasia dolens*. The affection of the saphena vein they observed to be as frequently subsequent as concomitant to that of the limb generally, which they regard as an inflammatory state of the subcutaneous cellular tissue. The reporters subjoin a case in which, with marks of bronchitis, pleuro-pneumony, *pericarditis*, and *splenitis*, the inner membrane of the *vena cava* was found inflamed, and its cavity obstructed,—a combination of morbid changes which Mr Arnott, and Messrs Velpeau and Daudin might employ in illustration of their respective doctrines.

Under the head of disease of the lymphatics, is related the case of a young woman of nineteen, who died after twenty-nine days' illness, and on inspecting whose body there were found a large abscess of the *psoas* muscle, tubes proceeding to the pelvic glands, containing purulent matter; the glands distended with similar substance, though more consistent; the thoracic duct distended to the size of the little finger, and its tunics hard and nodulated; and the uterus filled with caseous matter. This case gives occasion to sundry remarks on the probable duration of the lumbar abscess, on the characters of the minute tubes which opened into its cavity, and the question as to the deposition, in solid or fluid form, of tubercular matter. The reporters might have expressed themselves with perfect confidence as to the long duration of the abscess; for it is by no means uncommon, indeed, it may be said to be the regular course of things, for a lumbar abscess to attain a large size without producing the smallest effect on the health; and the circumstance of its not being detected till after death, though examined by several surgeons, is by no means singular. It is manifest, nevertheless, that the illness with which the girl was attacked about a month before death, and which continued in various degrees of regularity till that event, was the consequence of the secondary or acute inflammation which sooner or later supervenes in the chronic abscess. On the question of the fluid or solid form of the deposition of tubercles and tubercular matter, the reporters are perfectly correct; and we only wonder how they could think of troubling themselves with any examination of opinions so absurd as those which Andral has allowed himself to adduce. They show at least a very good-natured deference to his authority, but one which is by no means merited. We lay it down as a point most clearly and indisputably proved by the phenomena of inflammation and lymphy deposition traced in numerous instances, that all morbid products whatever are originally deposited in the fluid state, and that their consolidation is a subsequent process, partly chemical, partly vital.

The diseases of the respiratory system comprehend the usual large proportion of affections of this class in these cold and variable climates. Pleurisy, simple and uncomplicated, is rare in Dublin; but acute inflammation either of the bronchial mucous membrane or of the pulmonic tissue is common. The pathology, semiology, and treatment of these diseases have been sufficiently established; but the authors of the present report have merit in accommodating with precision the exhibition of tartar emetic to the stage of the disease. According to their experience, it is during the first stage of pneumonia, while the crepitating rattle is distinct, and before the inflamed portion of lung sounds dull on percussion, that the remedy is most efficient. The dose administered the first day is six grains; and it is increased by two or three grains daily, until the quantity administered in twenty-four hours amounts to fifteen grains. Beyond this they never found it necessary to go; and they could persevere in its exhibition at this rate for many days with the best effects. It is a remarkable circumstance in the operation of this agent on the human body, that, though nauseating and emetic in small doses, it becomes quite endurable in large ones during the presence of pneumonic inflammation. In one case of double pneumonia supervening on bronchial inflammation, 180 grains were exhibited at the rate of twelve grains daily; and after the second or third day the endurance was completely established, and towards the termination of the disease the appetite was good. The usual effect is to induce nausea without vomiting; and this condition continues for several days. In a few cases full vomiting took place at first, and in some vomiting and purging; but these effects subsided, and were followed by mere nausea. Diaphoresis is rare; and very often little change could be recognised in the secretions. The pulse, nevertheless, gradually fell, and the stethoscopic symptoms improved. Intestinal irritation they rarely observed to follow its use; and when it did, the pains yielded to laxatives, opiates, and fomentations. In one case only did they require blood-letting, when the blood was buffed and cupped; but recovery was complete.

There is, however, a class of cases, those in which the pneumonia is complicated with *gastro-enteria*, in which the remedy cannot be endured, and is resisted by the disease. In cases of this description, where the abdominal pain or diarrhœal discharges give reason to suspect the existence of inflammation of the intestinal mucous membrane, the latter must first be removed by the liberal application of leeches to the belly, and the use of tartar-emetic is then to be resumed.

Besides tartar emetic the reporters place great confidence in the use of calomel and opium in pneumonic inflammation; but they have not succeeded in determining the kind of cases to which each remedy is most appropriate. They are inclined, however, to employ the emetic in cases of simple pneumonia occurring in robust habits, and accompanied by inflammatory or sthenic fever with much tension of the system, and to prefer the latter in cases in which the pneumonia is complicated with intestinal disease, and where the fever assumes the typhoid or asthenic type with general depression of the vital powers. They add, that where partial condensation of the lungs have taken place previous to application for advice, the mercurial plan, aided by local blood-letting and the use of blisters, has been that which is most efficacious. After the first stage of the disease is over, the emetic has little efficacy; but under the plan by calomel and opium they have witnessed the rapid resolution of extensive solidified portions of lung.

These observations confirm the efficacy of the method practised more than thirty years ago by Dr Hamilton of Lynn Regis, and which we cannot help thinking has been most unjustly neglected. By many, indeed, it is almost unknown; and some practitioners, when the method which Hamilton practised, of administering calomel and opium for the cure of severe pneumonic inflammation, is mentioned, exhibit much the same sort of countenance as if such a remedy was stated to be used by Hippocrates or Aretaeus. Though the diagnosis of pneumonic diseases was not then so accurately established as it now is, and consequently, the symptomatic characters given by Hamilton partake of the vagueness common to these affections, yet it is manifest, from the kind of pneumonic affections to which he appropriated this mode of treatment, that they correspond closely with those designated by the present authors. This method, indeed, Dr Hamilton appears to have employed principally in severe and advanced cases of the disease, in which blood-letting was either unsafe or could not be carried the requisite length; and he employed it with great advantage, he states, in pneumónia occurring in pregnant women.[*]

According to the experience of the present authors, pneumonia in the third stage is very generally fatal. In one case, however, recovery took place; and, though the lower portion of the left lung emitted a perfectly dull sound, it first presented the phenomena of a cavity with purulent expectoration, and finally became clear, and emitted the respiratory murmur. Some judicious remarks are made on the disappearance of the phenomena of hepatization, without the occurrence of the resolving crepitus, and on the occurrence of bronchial respiration during the subsidence of the disease.

The report contains an instance of encephaloid tumours in the posterior mediastinum and left lung, which gave rise to stethoscopic signs exactly similar to those of tubercular masses. Their presence appears to have occasioned bronchial inflammation. An observation, that in several cases of phthisis in which the chest sounded every-where well during life, yet on inspection the lungs were found generally solidified from tubercular disease, shows still how inefficient the practice of auscultation and percussion is, and how cautious the practitioner must be in his opinion and measures, even after he has examined the chest in this manner.

Laryngeal inflammation, especially of the chronic form, appears to be a common disease in Dublin,—a circumstance which appears to be referable, not only to the neglect of the lower orders, when exposed to cold and moisture, but to the frequency of constitutional syphilitic taints. That the *laryngitis* from the latter cause is both distressing and unmanageable, we can testify from personal observation, having had occasion to see several cases resulting from this cause proceed to the fatal termination. The treatment found most successful, in addition to the antiphlogistic and counter-irritant measures in both cases, is the use of mercury, and the application to the parts, so far as they are accessible, of a strong solution of nitrate of silver, by means of a hair pencil.

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[*] Letter from Dr Robert Hamilton to Dr Duncan, giving an account of a successful method of treating inflammatory diseases by Mercury and Opium.—Medical Commentaries, Vol. ix. p. 191.

A case of diaphragmatic hernia, in which the cardia, stomach, pylorus, and transverse colon were protruded into the left *demithorax*, and thrust the heart and mediastinum to the right side, is adduced to show a new source of difficulty of distinguishing pectoral diseases, since, in such circumstances as those now mentioned, the stethoscope applied to the left demithorax would have given different indications, according as the stomach and colon contained solid matter or air. From this case also, the reporters infer, that vomiting may be produced by the action of the stomach alone, without any compressing power from the diaphragm, as is maintained by Magendie. The reporters are mistaken, however, in ascribing this doctrine to Le Gallois.

The portion of the report relating to the diseases of the abdominal viscera is subdivided into two parts,—one relating to diseases of the liver, the other to the affections of the intestinal tube, especially *peritonitis*. Under the first head are given five cases of hepatic abscess,—a disease which, the reporters justly remark, has been frequent in Dublin during the last year. All the cases detailed were fatal, and the necroscopic appearances are annexed. The first two of these cases are interesting in this respect, that in the first, after the tentative incision made in the manner of Dr Graves, a second puncture made by a lancet divided the gall-bladder; and, though the patient, in the meantime, partially recovered, death took place in consequence of secondary peritonitis resulting from the escape of matter from the left lobe into the peritoneal cavity. In the second, an attempt was made to open the abscess by the ulceration produced by means of the slough of caustic potass. On the detachment of the latter, a puncture by a lancet gave vent to a few ounces of purulent matter. But the operation was followed by severe peritoneal inflammation terminating in albuminous effusion and adhesion. At the end of twenty-seven days death took place. Yet no fluid was found in the peritoneal cavity, and the only trace of inflammation consisted in the presence of numerous organized adhesive membranes between the intestinal convolutions. The fifth case is also remarkable in terminating by the formation of a fistulous canal, extending through the lower lobe of the right lung, and opening externally between the eighth and ninth ribs.

The cases of intestinal disease are chiefly perforation of the intestines from previous disease of the mucous follicles, and consequent fatal peritonitis. They are good examples of this enteric disease, and its peculiar termination.

The articles constituting the second part of the volume may be arranged in the following manner; anatomical papers, physiological, anatomico-pathological, therapeutic, and surgical.

The first anatomical paper which we shall notice is entitled, *Observations on the Mucous Membrane of the Rectum*, by J. Houston, M. R. I. A., &c. In this the author shows that the mucous membrane is doubled into three or four semilunar folds, which he distinguishes by the name of valves. The largest and most uniform of these is about three inches from the anus, opposite the base of the bladder; the next in size and uniformity of presence is at the upper end of the bowel; the third is about midway between the first and second; and the fourth, when present, adheres to the side of the bowel about one inch above the anal outlet. The intermediate spaces between these valvular folds is occupied by others smaller in size, and less regular in situation.

The convex margins of these folds are attached to the rectum from one-half to one-third of the circumference of the bowel, and their free concave margins, which are defined and sharp, are generally directed upwards. Their breadth at the middle varies from one-half to three-fourths of an inch; and their taper extremities are gradually lost in the adjoining membrane. Besides mucous membrane and filamentous tissue, they are said to contain a few muscular fibres.

It is remarkable that the relative and successive situation of these folds constitutes a sort of spiral within the rectum. Thus the first or uppermost one projects from the right wall of the bowel above; the second, or intermediate, from the left; the third, or recto-vesical, the most uniform, projects from the anterior wall; and the anal, or inferior, which is least constant, from the left and posterior wall. From this arrangement, it results that the rectum, though similar in internal arrangement to the colon, approaches, nevertheless, to the large intestine of the lower animals, or the spiral valve of the serpent and dogfish.

The physiological purpose of this arrangement appears to Mr Houston to be chiefly to resist the weight of the intestinal contents, and prevent them from pressing on the anal opening, where their presence induces the uneasy sensation requiring their expulsion. Their influence in the production of several of the diseases of the rectum is, he also thinks, not inconsiderable. While, on the one hand, they appear to be not unfrequently the seat of stricture, on the other, their presence, he observes, must not be mistaken, as there is too much reason to suppose they have been, for that disease.

For an account of the other anatomical paper, containing the description of *two new muscles for compressing the dorsal vein* *of the penis in Man and some Animals*, we refer to the Medical Intelligence of the present number.

The first physiological paper which we shall notice, consists of *Observations on the Utero-placental circulation, and the phenomenon of the placental soufflet, with its utility in detecting the existence of pregnancy, and the death of the fœtus in utero*, by Dr Evory Kennedy.

It is well known that Kergaradec, and, soon after, Laennec himself, employed the stethoscope to recognize the presence and intensity of two kinds of sound in the uterine tumour during pregnancy, one, that of the placental circulation, the other, the beat of the fœtal heart. Though there is no doubt that these sounds really exist, yet, since they are transmitted to the ear of the auscultant with various degrees of intensity, according to the position of the *placenta* and the chord, it has happened that they have not been heard with equal distinctness by all. Fodera, for instance, though he admits the use of the stethoscope from motives of delicacy, contends that, in general, the sounds are more distinctly heard by the naked ear; and Siebold, the celebrated German writer on obstetric medicine, informs us, that not only could he hear nothing by the instrument in numerous experiments, but that by the unaided ear he heard with perfect distinctness the double beat of the fœtal heart, and the single whizzing sound of the placental circulation. The object proposed by Dr Kennedy in the present paper is chiefly to determine what are the actual facts of the case; to ascertain what sounds are heard in the uterine tumour, what are their causes, what varieties they present, and what are the circumstances on which these varieties depend.

Omitting an account of the distribution of the placental vessels, which, indeed, is virtually that of Hunter, and therefore contains nothing new, Dr Kennedy states his conviction that the phenomenon named the placental thrill *(soufflet)* depends principally on the transmission of blood through the arteries of that part of the womb to which the placenta is attached, and partly on the transmission of blood through the arterial tubes and cells of the uterine part of the organ. The reasons on which Dr Kennedy advances this opinion are the following. 1*st*, The placental sound is present in pregnant females only when the utero-placental circulation exists, and ceases when the vessels which perform this office become impervious. It is distinct and energetic immediately before parturition; but immediately after, when the womb is empty and contracted, or when the fœtus has died *in utero*, it is gone. 2*d*, It is periodical, alternating with intervals of cessation, and corresponding and synchronous in every case, with the pulse at the wrist of the mother. *3d,* The sound is always heard in that part of the womb only where the placenta is or has been attached,—a fact which Dr Kennedy has proved by manual examination during the removal of the placenta, and by inspection after death. *4th,* Dr Kennedy has recognized the same sound in the pregnant cow, the only animal exhibiting similar characters in the circulation of the placenta to that of the human body.

Admitting the soundness of these conclusions, Dr Kennedy further infers, that they prove that this phenomenon depends on the transmission of blood through arterial not venous tubes, and that these tubes are situate at that part of the womb where the placenta is or was attached. He contends that it cannot be occasioned by the transmission of blood through the arterial system of the fœtus, the fœtal part of the placenta, or the umbilical arteries, because the periodical impulse of the blood in these parts is nearly double the frequency of the placental motion or the pulse of the wrist of the mother; and that it does not depend on the passage of blood through venous tubes, must be evident from the fact, that in these both in the mother and fœtus the blood flows in a continuous not impulsive stream.

That the placental thrill depends chiefly on this motion of the blood in the uterine arteries, and with less probability on those of the maternal portion of the placenta, Dr Kennedy thinks may be inferred from the following circumstances. *1st,* When the *fœtus in utero* dies, the placental thrill becomes altered in character without being extinct. From several cases, two of which are given in detail, the author shows satisfactorily, we think, that, when the fœtus expires within the womb, and before the placenta is detached, there still continues a thrill, abrupt, however, and short, and void of the sibilous lengthened sound by which the placental circulation in its healthy and entire state is distinguished. The reason assigned for this change by Dr Kennedy is, that the utero-placental blood, instead of passing through the placental arteries, is taken up by the numerous anastomosing vessels, and conveyed back to the uterine veins and sinuses without circulating through the placenta.

Dr Kennedy concludes this part of his inquiry by considering and refuting the objections urged against the assigned cause of the placental thrill by Haüs and Dugés. These objections, which proceed on the assumption, that there is no connection between the arteries of the pregnant womb and the placenta,—a point, the affirmative of which was established by Hunter, and which may be readily demonstrated by injecting the uterus of a female dead during pregnancy, may be disregarded entirely; and though we admit that it is incumbent on the author to expose their fallacy, it is unnecessary for us to dwell on what implies an anatomical error. On the whole, Dr Kennedy main-

tains that, though it would be rash to assert that auscultation alone affords unequivocal proofs of pregnancy, the presence of the placental thrill in an abdominal tumour, combined with the double and frequent beat of the fœtal heart, always indicates pregnancy. It is, nevertheless, manifest, that if the placenta be attached to the posterior part of the uterus, especially towards its neck, the placental thrill may be beyond the reach of the instrument; and the author admits that this may probably have been the reason that it was not recognized by some observers in cases in which the sequel afforded unequivocal proofs of pregnancy. The recognition of the placental thrill is chiefly important as a diagnostic mark in the early stage of pregnancy. In the advanced period, when the action of the fœtal heart is strong and distinct, it is of much less moment, since the latter circumstance alone is sufficient to decide the question. To show the degree of reliance which may be placed on the former as a diagnostic mark, the author adduces the following case.

"I was sent for one morning by a lady who came over clandestinely from the sister kingdom. The statement she gave me was as follows: She had for some months past been in the habit of receiving the addresses of a gentleman, for whom she had formed an attachment, but unfortunately fell a victim to her father's caprice, who, after countenancing this attachment, suddenly withdrew his consent to their union, and insisted on her marrying an individual of his selection. Ten weeks had elapsed from her first giving way to illicit intercourse, when she consulted me; during which two monthly periods had passed without the usual menstrual discharge. About ten days before my seeing her, in consequence of some active exertion, a discharge of blood took place from the vagina, which lasted for a few days. Her father urged a compliance with his wishes, and she, dreading to enter the marriage state whilst there was a possibility of her being pregnant, consulted a medical man of eminence, who, after the usual investigation, pronounced that such was not the case. Impressed, however, with a painful foreboding of the true nature of her state, although she had no further symptom of pregnancy than that mentioned, she determined on obtaining further advice, and, under pretence of visiting a friend in the country, came over to Dublin. On the most accurate examination, I could ascertain no further grounds for suspicion, than the presence of a remarkably distinct *soufflet,* which was discoverable on pressing the end of the instrument on the pubic region over the uterus. Relying on this, I gave her to suppose that there was a strong likelihood of her being pregnant, although I could not actually pronounce such to be the case. The result justified the confidence I reposed in this as a means of diagnosis; for exactly nine months from the period when she calculated, she gave birth to a child."—Page 259-260.

Though it is only in the early period of pregnancy that the placental thrill becomes important as a diagnostic mark, it is not cognizable, according to the experience of Dr Kennedy, till after the second month from impregnation, but is distinctly so in the tenth, eleventh, and twelfth weeks. On the whole, Dr Kennedy conceives that by no one who takes pains to examine accurately the abdominal tumour, and to distinguish the sounds communicated, can the value of this as a diagnostic method be denied.

There is one class of cases particularly in which the stethoscopic examination of the uterine tumour acquires peculiar importance, those namely in which, with enlargement believed to indicate *ascites*, ovarian dropsy or some species of *physconia*, there are combined such circumstances as justify the suspicion that the individual is also pregnant. Such cases we have seen; and such have actually been submitted for examination to determine the propriety, when the tumour indicates the presence of fluid, of puncture. In such circumstances, the necessity of distinguishing accurately the fact of pregnancy becomes of the utmost importance, as the mistake must be a dangerous one, and may peril two lives. A good case of this description, in which the operation was on this ground deferred, is given by the author. But, short even of the question of *paracentesis*, it may be of the utmost moment still to determine the fact of pregnancy. A female, perhaps not over strict in her moral ideas, applies to a practitioner with an abdominal tumour, in which obscure fluctuation and previous history concur to give some colour to her story, when she represents herself as labouring under dropsy only. Whatever suspicions he states are repelled with a tone of virtuous indignation which makes him unwilling to injure the feelings of his patient. He gives up the point in the meantime, and exhibits remedies calculated to excite the action of the kidneys and remove the fluctuating tumour, and among others mercury. The urinary secretion is augmented; but the tumour does not subside in proportion; and after a few more weeks premature labour comes on, and the great bulk of the tumour disappears on the expulsion of a dead child. In this case, which we have actually witnessed, the abortion is induced by the use of the mercurial medicine, and the mother is the conscious agent who renders the medical practitioner the instrument of abortion. In such circumstances, it is impossible to be too cautious in adopting any measures on the supposition that the dropsical tumour is uncomplicated with pregnancy.

Another advantage resulting from the use of the stethoscope in ascertaining the existence of the placental thrill is, that it enables the accoucheur to pronounce on the life or death of the *fœtus in utero*. He is well aware, that, though he is always regulated in his measures by the state of the mother, yet, in many instances in which he hesitates, he would instantly proceed to deliver, were he quite satisfied that the infant was already lifeless.

Lastly, though the author does not express himself confidently on this head, he throws out the hint, that the stethoscopic examination of the uterine tumour, and the ascertaining the presence or absence of the thrill, might be of use in those cases of hemorrhage in which the attendant is uncertain as to the extent or rapidity of the evil, by reason of the want of external hemorrhage, and in those cases in which Dr Gooch recommended the firm compression of the placental part of the *uterus* between the hand introduced and the other applied to the abdomen. The thrill, he remarks, is observed in afterpains,—a circumstance easily explained. It is probable, also, that it may be heard during the flowing of blood from the uterine vessels into the cavity; and in this case, if in such alarming circumstances, when a few minutes decide the fate of the patient there be time and coolness for examination, it is not improbable that it might afford satisfactory information of the state of the uterine circulation.

The second physiological paper to be noticed at present, is by Dr Graves *on the varieties produced by posture in the frequency and character of the Pulse in health and disease.* It is known that considerable variety, both in strength and frequency, is observed in the erect, the sedentary, and recumbent posture. Dr Thomson of this place first pointed out some of these differences from an extensive series of accurate observations; and these were confirmed by Dr Stroud in the numerous observations on the patients of the clinical wards in the years 1816 and 1817. Unaware of these experiments, apparently, Dr Graves has, with extreme diligence, devoted himself to the observation of the varieties of the pulse not only in the erect and recumbent posture, but in the inverted, with the feet above and the head dependent. The author found, however, that this position produced much less effect than he anticipated. The frequency of the pulse in this position was neither retarded nor accelerated; but its strength was diminished sometimes very considerably, and occasionally it became very irregular,—a circumstance which Dr Graves is inclined to ascribe to the weight of the column of blood pressing on the aortic valves, and thus augmenting the impediment to its exit from the ventricle.

Dr Graves finds the pulse less frequent, from six to fifteen beats in the minute, and stronger in the horizontal than in the erect posture; and he infers, that in this position its *maximum* of strength and *minimum* of frequency are at once attained. This fact, he thinks, may explain the relief obtained by placing patients in the horizontal position in order to avoid fainting, for instance that produced by venesection. This, however, with deference to the author, we must be permitted to question. Syncope from venesection is doubtless occasioned by the blood being so suddenly drained off, that there is no longer a sufficient quantity left to excite the action of the heart; and it is with equal certainty obviated by placing the patient in that position in which, while the blood is drained from the superior vessels, it may, by being gradually allowed to flow from the superior ones, still be adequate to excite the heart.

In all diseases, excepting six cases of hypertrophy with dilation of the heart, Dr Graves found the pulse to differ in frequency in the erect, sitting, and horizontal postures. In four of these six cases, the fact of hypertrophy with dilatation was ascertained by inspection; and of the other two, in one it was certain during life, and in the other at least probable. It would be doubtless premature to speculate on the cause of this peculiarity until the phenomenon has been observed in a greater number of instances. We conclude our account of this paper by subjoining the general conclusions, which Dr Graves thinks may be established from an extensive number of observations.

" *1st,* That the greatest difference occurs in patients labouring under fever or in a debilitated state, in consequence of fever or any other cause. It may amount to thirty, forty, or even fifty between the horizontal and erect postures.

" *2d,* That this difference decreases after the first quarter of an hour in most cases, but always remains considerable as long as the same position is observed.

" *3d,* That, in persons not much debilitated, the difference is much less than that stated above, and often does not amount to more than ten.

" *4th,* That, when the patient lies down, the pulse rapidly falls to its former standard.

" *5th,* That in some the frequency is greater between the horizontal and sitting posture than between the latter and the erect; while in others the contrary takes place, so that generally the frequency in the sitting posture may be taken as a mean.

" *6th,* In persons convalescent from fever or acute diseases, I find it is extremely useful to the physician to ascertain the comparative frequency of the pulse in the horizontal and the erect position. The greater the difference, the greater is the debility of the patient; and, consequently, the more guarded must his medical attendant be in allowing him to sit for any length of time, particularly if the pulse, in his lying down, does not resume his usual degree of frequency."—P. 570.

On the cause of this change in the frequency of the pulse in the erect and recumbent position, Dr Graves admits his inability to advance even a conjecture. He connects the general fact, however, with that observed by Humboldt in the hearts of frogs cut from the body, and with the large vessels tied, that the pulsations were eight fewer in the minute when laid on a plate of glass than when suspended perpendicularly.

Of the papers on pathological subjects, the first that may be noticed as a sequel to the Report of Dr Graves and Stokes, is an interesting case of aneurism of the abdominal aorta by Dr Beatty.

The difficulty of recognizing this disease is well known; but perhaps in no instance is it more forcibly illustrated than in the one recorded by Dr Beatty. The individual was a gentleman aged 33, of robust frame, and regular and temperate habits, who was attacked, after exposure to cold, with dull pain in the loins, which was thought to be *lumbago*. He was first seen by the author, who, with Dr Cheyne, employed antimonials, mercurials, narcotics, purgatives, local cupping, and blistering, &c. without decided benefit. In September the same year, he proceeded to Cheltenham, where, under the care of Dr Boisragon, he used the waters for about six weeks, with occasional doses of blue pill and rhubarb, employed the warm bath and friction by the flesh-brush, and lived rather freely. In November, when he returned to Dublin, he was free from pain, and improved in appearance; but within a week the pain recurred in the form of severe spasmodic twisting of the whole tract of the intestines, particularly in the region of the colon on both sides, with dull, heavy, dragging pain, referred to the whole abdomen, aggravated in bed, and by external heat, and relieved by getting up. At this time, under the care of Dr Graves, he underwent a course of turpentine injections and blistering, with temporary relief, and lived rather freely, always taking wine. He continued much in the same state during the winter, and it was only in April 1828 that his health showed symptoms of decline; his appetite became bad, and his strength was impaired. He now resumed the use of the warm bath; but in consequence of the pain being aggravated, he was obliged to abandon it for that of the cold bath, joined with *cinchona*, which, however, from disagreeing with the stomach, was soon given up. At this time, when he was often seen by Mr Colles, he took exercise on horseback and in a gig, but was occasionally attacked with evening rigors of some severity.

After remaining much in the same state, with occasional aggravation of his sufferings, for the rest of the summer, he was advised by Dr Beatty, about the end of August, to proceed to Paris, to obtain the opinion of M. Andral on the nature of his disorder, on which no definite opinion had yet been formed by the Dublin practitioners. This measure he effected, and arrived in Paris about the middle of September, when, with Dr Graves and Mr Townsend, after a minute examination by the Parisian physician, the latter stated his opinion, that there was no indication of organic disease; that the symptoms were caused by an intestinal *neurosis*; and that there was great analogy between them and those complained of by persons under the influence of lead. During his residence in Paris, which continued till the beginning of October, he improved and was free from pain; but while landing at Dover, he was once more suddenly attacked with violent and excruciating pain from the back, through the bowels, and which lasted for some hours. Opiates afforded relief; and the following day he reached London, where he consulted Dr Wilson Philip, who ascribed the complaint to indigestion, and the spasmodic pains to the disengagement of gas from indigested food. Here he remained three weeks, using remedies calculated to improve the digestive organs, and evidently recruited under their use. Towards the end of October, he left London, and in a few days arrived in Dublin, where he resumed his professional labours. The pain and spasm, however, soon returned, and now, for the first time, affected the chest. The symptoms, which assumed a more permanent character, consisted of pain in the epigastric, right hypochondriac, and thoracic regions; the abdominal pain prevented him from using the recumbent posture, and he sat constantly in a chair supported by pillows; and though blood drawn at this time exhibited the buffy coat, his pulse was only 80, and he practised singing every evening without inconvenience. His abdominal pain in the meantime became more permanent and constant, and consisted of excruciating spasms darting through the spine and dorsal muscles, and upwards to the scapula. These sufferings were occasionally relieved by strong coffee and preparations of opium; and he also took carbonate of iron without advantage, and sulphate of quinine with considerable benefit. Some days after, however, on the 15th January, he awoke with pain in the left side, rapidly increasing and producing intense distress, while his pulse rose to 100, and never again fell. Blood was taken from the arm, mercury was given to ptyalism, and large doses of opium were exhibited to assuage the severity of his sufferings.

On the 1st of February, he was suddenly seized with pain in the left groin and near the crest of the ilium, extending to the back and belly, the muscles of which were affected by severe spasms. These were relieved by large doses of the black drop; but after gradually becoming more constant the respiration became difficult; and after three weeks more of increasing weakness, he expired in a state of convulsion and paralysis.

Inspection showed the liver not to be enlarged or otherwise diseased, but forcibly compressed against the ribs by a firm tumour as large as the head of a child of twelve months, having the aorta passing down its anterior surface in a groove or furrow. On dividing the artery at the bifurcation, and slitting it up above the tumour, its posterior surface presented above the origin of the cœliac artery, a round well-defined hole as large as a shilling, communicating with the tumour, which was thus proved to be an aneurism of the aorta. It was covered by the *crura* of the diaphragm, which stretched tightly over its surface formed an outer coat, in which numerous nervous filaments were observed. The upper part on the left side presented an irregular opening, through which the blood of the tumour escaped into the pleura. The posterior surface of the sac, which was wanting, pressed the three last dorsal vertebræ, which were deeply eroded, while the cartilages were entire; and on the left side of the eleventh dorsal vertebra was a hole large enough to admit a finger, leading into the spinal canal. It is almost superfluous to add, that the stomach and bowels were comparatively healthy.

After the statement of the principal facts of the foregoing case, we conceive it unnecessary to enter into any of the questions which naturally suggest themselves to the author, and which, we allow, he discusses with ability regarding the obscurity and difficulty of the diagnosis. That the cases perplexed so many able observers in Dublin only shows how difficult it is in some cases of organic disease to form any distinct idea of their nature. That it was regarded by M. Andral as an intestinal *neurosis* is by no means wonderful; for while this name is made the convenient head of reference for every painful disorder of which it is difficult to determine the exact locality, it is not singular that a complaint so obscure in origin, and progress, and locality, should be pronounced to belong to this class of maladies. From the whole case, indeed, it results that the diagnostic marks hitherto given of aneurism of the abdominal aorta are completely equivocal, and of no use.

From the case which occurred in the Meath Hospital, indeed, and one or two others, Drs Graves and Stokes infer, that one of the most constant symptoms is pain in the back. But the variable character of this pain, not only in this case, but in that of Dr Beatty, shows how difficult it is to render this a precise symptom. As to the symptoms of painter's colic, with which it is represented to be attended, these, we can assert from observation, are common to every species of abdominal tumour, in which the intestinal tube, or any part of it, is much pressed or stretched by the increasing size of the body. Thus it occurs, we know, in enlarged kidney; and, what is remarkable, it is similar to that observed in Dr Beatty's case, in coming in fits and periodical accessions. The disorder of the stomach is certainly not common; and its occurrence in the case of the man Hamilton in the Meath Hospital, is doubtless to be ascribed to its vicinity to the stomach itself. On these symptoms, however, it is impossible to reason with any confidence, since dissection was not permitted.

Another important deduction resulting from this case is, that in no case in which the nature of the affection is undetermined, ought the physician to recommend therapeutic measures, which he is not sure may operate in aggravating the disease, and render thé sufferings of the patient more dreadful. The thanks of the profession are particularly due to Dr Beatty for the candid and accurate detail which he gives of its progress and termination.

The next pathological subject which may be noticed is that of *Rupture of the Intestines from contusion*, by Mr Hart. This occurred in the person of an hostler, aged 36, in consequence of receiving a kick from a horse in the belly, which induced immediate fainting. On recovering from this he felt acute pain in the belly, which in a few hours augmented to an excruciating degree, and was attended with constant vomiting, while the pulse became small and frequent, and the countenance anxious and hippocratic. Death took place in eighteen hours. Inspection showed a transverse, oval-shaped rupture dividing the upper part of the *jejunum* half across, allowing the contents of the intestines, with bile, to escape into the peritoneal cavity, and producing inflammation, with copious effusion of lymph. The arch of the colon was marked by a dark-coloured patch, the effect of ecchymosis; but the intestine was not ruptured. Between the folds of the transverse mesocolon and behind the root of the mesentery, was a quantity of dark-coloured blood, amounting to 16 ounces, which had escaped from the extremities of the lacerated mesenteric vessels.

The peculiarity in this case is the laceration of the intestine without breach of the abdominal parietes; and this Mr Hart ascribes to the fixed situation of the upper end of the jejunum with the solid spine behind, on which the impinging force, he conceives, necessarily impels it; and, for the same reason, he con-

roid cartilage. The hemorrhage was inconsiderable ; yet the pulsations of the heart were imperceptible, the limbs were cold, and consciousness was gone. The reason of these symptoms was, that the epiglottis detached as above stated, had fallen back over the *rima glottidis*, and, by intercepting the passage of air to the lungs, had produced temporary *asphyxia*. By bringing the epiglottis forward, and securing it to the anterior surface of the thyroid cartilage by a single point of suture, the chest began to move, respiration returned, the action of the heart was re-established, and sense and motion returned. The patient, however, never recovered from the fit of derangement in which he had perpetrated the act ; and erysipelatous inflammation of the neck and throat terminated his existence in the course of a week.

Mr Houston next details two cases of malformation of the heart. In the first, a boy of three years, who died in the Meath Hospital of the effects of bronchitis, tubercular excavation of the lungs, and infiltration, the pulmonary artery was only abou[illegible]lf the usual size, and had only two semilunar valves ; and a defined smooth hole, capable of admitting the little finger, was found in the *septum*, establishing a communication between the right ventricle and the upper part of the left. The left auricle was small, the left ventricle of the same size as the right, and the aortic semilunar valves were perfect. The *aorta* was unusually large, and the *ductus arteriosus* diminished, but not obliterated. In the second, an infant, who died after frequent convulsive attacks with lividity, in one of the struggles, the *foramen ovale* was round and open, the right ventricle was larger than the left, almost void of *columnæ carneæ*, and, instead of the passage into the pulmonary artery, which was completely obstructed, a wide opening led into the aorta, which thus received the blood of the right ventricle. A large round aperture, also in the upper part of the *septum*, opposite the passage into the aorta, established a free communication between the two ventricles. The aorta was much dilated at its origin ; and the *ductus arteriosus*, which was large and pervious, might be said in this instance to rise from the aorta, and to convey blood to the lungs, instead of the pulmonary artery, which was obstructed.

These cases belong to that class which have been so well described by Hein, and which are best distinguished by their physiological character, of allowing the venous to communicate directly with the arterial blood. *

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* De Istis Cordis deformationibus quæ sanguinem venosum cum Arterioso misceri permittunt. Gotting. 1816.

An instance of *hydrocephalus*, in a child of one year and seven months, is chiefly distinguished by containing, not only serous fluid in the ventricles, but not fewer than six cysts of different sizes, each containing limpid fluid, in different parts of the brain.

In a valuable paper, Dr Robert Law gives several good examples of some of the most remarkable organic diseases of the brain. Thus, there is a case of hard tumour in the right hemisphere, with surrounding softening; an instance of the general prevalence of the *tyromatous* or tubercular diathesis in the circumstance of a tumour of tyromatous matter in the left posterior lobe of the brain, tyromatous pisiform tubercles in the lung and intestines, and similar substance in the suspensory ligament of the liver; an instance of *caries* of the cranium, with pulpy destruction of the brain; a case of abscess of the *cerebellum*; and one of pulpy destruction of both optic chambers and striated bodies.

Dr Crampton records an interesting case of inflammation of the *vena cava*, and the iliac and femoral branches, wi[illegible]albuminous exudation filling their canals, and extending upwards into the *venæ cavæ hepaticæ*, and as far as the connection of the *cava* with the right auricle. The value of this case consists in the fact, that while the inflammatory action appears to have originated in the *cava*, and spread upwards and downwards, producing the usual morbid secretions, when it arrived at the femoral veins it produced a general diffuse swelling of the whole right leg and thigh, accompanied with much pain on pressure, and assuming exactly the appearance of *phlegmasia dolens*. The venous inflammation appears to have been occasioned by the pressure of a tumour, which involved the *vena cava* from its connection with the auricle, to within an inch of the part where it was completely obliterated. The case is also useful in showing the effect of obstruction to the motion of the blood in the venous canals in inducing dropsical effusion.

Lastly, Dr Montgomery, with the view of elucidating the pathological history and characters of cancer of the womb, describes the semiographic phenomena of eight, and the necroscopic appearances of six, instances of that unmanageable disease. These cases present nothing very peculiar, and the general result is, that all proceeded in the destructive process to the fatal termination. We must be allowed to remark, however, that, though the author states the history with sufficient detail, and notices the general destruction, he fails to give, what alone is desirable to know in such cases, a distinct account of the anatomical characters of the disease. He neglects, for instance, to inform his reader what is the form of carcinomatous destruction, whether tuberculated, cartilaginous, or lardaceous, and seems to belong to that class of pathologists who think they do enough when they tell us that the *os uteri* or its body is destroyed, and who paint in vague general terms "the ravages of this unmanageable disease." In the present day this is of no use, and cases of this description might as well be undissected, as described in this unsatisfactory manner. He even does not take the trouble to determine the manner in which cancer of the womb proves fatal; and though he disavows any practical object in his paper, he cannot be said to add a single pathological fact. These reasons induce us to doubt whether all the cases detailed by Dr Montgomery really belong to the head of cancer; and we require something more to establish the characters of this disease, than the general and vague assertion that they were so. As to the general symptoms enumerated by Dr Montgomery, in common with numerous other authors, of induration and lacerating pains, we have no hesitation in saying, that they are totally inadequate to justify the application of the epithet.

For one thing, however, Dr Montgomery deserves commendation. Though not negligent in the means of relief, he disclaims the idea of attempting to cure *cancer uteri* by any remedies, external or internal, by excision or by the use of iodine, or any other remedy. The latter substance he tried, and, as any person of common sense and knowledge of the powers of that substance might have predicted, without any good effect. Nothing, indeed, appears more singular than the absurd and irrational confidence which many practitioners, we must be excused for not calling them physicians, repose in this agent. It is a beautiful proof of the total independence of medical practice on good theoretical knowledge, and of medical prescription on the present zeal for cultivating pathology, that all those hopeless complaints which depend on positive change of structure are selected for the purpose of iodine exercising its sanative powers. With such modes of practice science and pathological anatomy have evidently no relation; and we must consign them to that blind species of experiment which hopes for success, not by the judicious trials which she makes, but by the previous mistakes which she commits in confounding a curable with an incurable disease.

Here, however, we must pause in our examination of this volume. Of the merits of the individual papers. we have already spoken. We can only say that the volume is altogether highly creditable to the physicians and surgeons of Dublin, and shows that their hospital officers are chosen in such a manner as to reflect some credit on the institutions to which they belong. One or two typographical slips we notice, which always make the reader suspect general inaccuracy. The name of Laennec ought not to be spelt Lænnec. We have already rectified a more serious mistake,—that which ascribes the idea that vomiting is produced by the diaphragm and abdominal muscles to Le Gallois. This hypothesis was proposed and defended not by Le Gallois but by Magendie.

For notices of the contents of some other papers in this volume, we refer in general to our Medical Intelligence, where they are referred to their proper heads.

Art. IV.—*Die Lehre von den Giften in Medizinischer, gerichtlicher und Polizeilicher Hinsicht, &c.*

Toxicology in its relations to Medicine, Law, and Police. By Dr K. F. H. Marx, Professor of Medicine in the University of Göttingen. Vol. I. Parts First and Second. Göttingen, 1827 and 1829. Pp. 270 and 580, 8vo.

Having concluded so very recently our notice of one systematic work on Poisons, it would be unjust to our readers if we were at present to occupy any material part of our pages by a detailed account of another on the same subject. Partly on this account, and partly too, because the portion of Dr Marx's system of Toxicology hitherto published is really of such a nature as to defy analysis, we shall confine the present notice to some general remarks on the kind of task the author has imposed on himself, and a short sketch of the manner in which he has executed it.

The work of Dr Marx is to consist of Two Divisions; the former being a historical exposition of all that has been hitherto written on General Toxicology; the latter a treatise on Special Poisons. The former division only has been hitherto published.

The Historical department of Toxicology has been hitherto much neglected. It has in particular been passed entirely over by the latest systematic writers in this country and in France. *M. Orfila* and *Dr Christison* have gone scarcely more than a century back for the facts and principles brought forward in their writings; and the latter expressly avows that he prefers modern to ancient facts on account of their superior precision and accuracy, and that such being the case he considers it unnecessary to incumber a practical treatise with mutilated facts and exploded doctrines from the writings of the ancient physicians. This appears undoubtedly a sufficient reason for excluding the History of Toxicology from a systematic work intended for practical purposes only. But at the same time the history of the science is by no means deprived of interest by the imperfect success of its earlier votaries in attempting its cultivation. We question, indeed, whether any other department of medical knowledge stood on a higher station in respect to the rest in ancient times than toxicology; and assuredly the lover of antiquity will have no difficulty in finding in the works of the Greek, Roman, and Arabian authors, as well as in the works of the earlier moderns, numerous hints and anticipations of what are too prevalently considered late discoveries, as to the operation of poisons, and the treatment of their effects on man. This has been ably shown in a late number of this Journal in a learned disquisition by *Dr Francis Adams* on the toxicological principles of the ancient physicians. An accurate historical exposition of the progress which our predecessors have gradually made in the knowledge of poisons,—their action, their effects on man, their antidotes, and their application to medical practice,—was therefore greatly needed. It was necessary as an act of justice to our forefathers in the profession; and it was desirable as the means of saving unnecessary expence of time and labour to succeeding toxicologists in the prosecution of farther discovery.

But the duty of writing such a history was no light task for any man to undertake. The elements of the work are scattered throughout such a multiplicity of authors in all departments of medicine, that even the short-hand method of reading through the medium of indexes seems scarcely to put it in the power of one man to come to an end within a moderate compass of time; and his search must so frequently end in irksome ill-requitted labour that we suspect few are to be found qualified for the undertaking who would not often be tempted to abandon it in disgust. The student of toxicology is therefore under deep obligations to Dr Marx, who has been the first to undertake the arduous task of making known the whole literature and history of the science, and to accomplish the important object of putting it in the power of any ordinary reader to make himself acquainted with every thing that each physician since the origin of the healing art has done to elucidate the effects of poisons and the proper methods of treating them. With a perfection of industry, for which his countrymen are peculiarly distinguished, he has apparently allowed no work, good, bad, or indifferent, to escape scrutiny, provided it was connected in the remotest manner with his undertaking; and he has recorded the sentiments of each author, sometimes in the original words, sometimes in abstract, with a fidelity for which, wherever we can follow him, we can conscientiously pledge ourselves.

The volume consists of a series of short sections, which may be called the text of the work, and constitute each of them a brief general view of the progress of one department or another of toxicology for a certain moderate period of time. At the close of each section are interposed between it and the succeeding one (according to the present prevailing fashion in German works of science) the *pieces justificatives*,—the documentary evidence in support of the statements in the text,—consisting of a chronological arrangement of analyses and extracts explanatory of the facts and doctrines to be found in every author of the slightest merit. These extracts and analyses form by far the greater part of the volume; and as they are printed in a smaller type than the text, they necessarily convey a prodigious store of information.

We shall be best able to convey some idea of the contents of the volume by the following general sketch of the subject of the sections.

He commences with explaining what has been usually understood by the terms *poison* and *poisoning*, what ideas the ancients entertained on the subject, and their opinions on the relations which poisons bear to medicines and aliments. A few remarks on the importance of Toxicology and on the various collateral sciences which constitute its elements close the Introduction. He then proceeds in quest of the earliest notice taken of poisons in ancient history, and treats of the poisoning of arms, of the use of poisons for the execution of criminals, of the modes employed by the ancients for the administration of poisons, of the methods resorted to for discovering them, and of the ancient punishment for the crime of murder by poison. He next endeavours to settle what information the Greek physicians possessed of poisons, and illustrates his observations by referring to the works of Erasistratus, Apollodorus, Zeno, Apollonius, Chrysermus, Andreas, Sostratus, Heraclides, Zopyrus, and Nicander. The Roman physicians then attract his attention; and Celsus, Scribonius Largus, Andromachus, Vindicianus, Dioscorides, Pliny, Galen, and Aretæus are quoted. After these come the later Latin and Arabian authors, Oribasius, Serenus Sammonicus, Ætius, Paulus, Johannes, Ælius Promotus, Geber, Rhazes, Avicenna, Serapion, and Averhoes. He then takes a survey of the state of toxicology in the middle ages when the practice of medicine was in the hands of the monks; makes some curious quotations from the Acta Sanctorum, illustrative of the treatment of poisoning in these barbarous times; expatiates a little on the frequency of murder by poison, which was at that period perpetrated even by poisoning medicines, and wells, nay, the sacramental elements; and treats of the punishment assigned for the crime of poisoning among the ancient English and Bavarians, the Visigoths, and, according to the statutes of King Roger of Sicily, the Emperor Frederick the Second, and Pope Gregory the Third.

From this period he advances to that of the revival of medicine, as aided by the laws of Charlemagne and Frederick the Second, the establishment of the Schola Salernitana, and of various public medicinal gardens, the enactment of laws at Paris for the visitation of apothecaries' shops, together with the writings of Gilbertus Anglus, Peter of Abano, Simon de Cordo, John of Gaddesden, Variguana, and Arnold Bachuone. Soon afterwards he traces the origin of a more systematic and scientific toxicology in the fifteenth century, as exemplified in the writings of Quainerius, Arduinus, Ponzetti, Champier, and Basil Valentine; and unfolds the effect of the Acta Carolina, the celebrated code of Charles the Fifth, in turning the attention of physicians in the sixteenth century to the medico-legal relations of poisons. A short digression here follows on the first introduction of poisons in modern times into the practice of medicine for the cure of diseases, especially syphilis; and this is succeeded a few sections farther on by another digression on the violent contests which took place as to the admissibility of poisons in the treatment of diseases, and on the prohibition of them by some governments. The enactment of the Acta Carolina, he shows, was soon succeeded by experimental inquiries on the operation of poisons, for which inquiries man, as well as the lower animals, was sometimes made the subject of experiment. Among the foremost in this line of investigation, are mentioned Matthiolus, Musa Brassavolus, and Conrad Gesner. At the same time, and about the commencement of the seventeenth century, many physicians treated elaborately, in their general writings on medicine, of the effects of poisons and their therapeutic virtues; among whom may be specified Fernelius, Vidus Vidius, Baptiste Porta, Fallopius, Hieronymus Mercurialis, Andreas Cæsalpinus, and others, less familiarly known in the present day. Physicians, however, were not at that time the only cultivators of this branch of medicine; unprofessional persons, and men of station, studied it for the worst of purposes; and, accordingly, the crime of poisoning became at this time alarmingly prevalent, as is exemplified in the histories of Pope Alexander the Sixth, Cæsar Borgia, and Philip the Second of Spain. Botanists and chemists, too, daily added to the stock of poisons, by discovering new plants and chemical compounds possessed of dangerous properties.

To meet the increasing frequency of the crime of poisoning, and the multiplication of the means of perpetrating it, medical men devoted themselves more and more to the medico-legal relations of Toxicology. Candanus and Ambrose Paré discussed the subject in express treatises; and not long afterwards, Fortunatus Fidelis, Paul Zacchias, Joannes Bohn, and Paul Ammann, laid the foundation in Italy and Germany of a regular system of medical jurisprudenee, of which the toxicological department formed a large and important part. About the same time physiological researches on the action of poisons began to be resumed; and, in particular, experimentalists conceived the idea of ascertaining the operation of poisons, by observing their effects when injected into the blood. Towards the close of the seventeenth and commencement of the eighteenth centuries, this method of investigation was introduced in England by Sir Christopher Wren, Charlton, and Courten, and followed up there and elsewhere by Moulin, Major, Elsholz, Etmuller, Zollikofer, Maurice Hoffmann, Wepfer, Fracassati, Baglivi, Redi, and others. These researches led about the same time to the recommendation of the practice of injecting medicines into the blood; and, besides, drew the attention of physicians more strongly than ever to the general therapeutic virtues of the active poisons,—giving rise, in consequence, to renewed and violent discussions as to the safety and advantage of employing such remedies. These researches appear also to have given origin to the theories of the mechanical physicians as to the action of poisons, which were published chiefly during the early half of last century, and among which the doctrines of Borrichius, Schrader, Mead, and Leeuwenhoek, are particularly specified by our author.

The commencement of the eighteenth century is also shown by Dr Marx to have been marked by a gradual and rapid advancement in the knowledge of the medico-legal relations of Poisons. More caution and scientific research began to be employed in the investigation of criminal cases; and through means of the example and publications of Vater, Valentini, Hoffmann, Zittman, Alberti, Gmelin, Etmuller, Bonetus, and Morgagni, the toxicological, in common with every other department of medico-legal knowledge, was raised to a respectable station among the medical sciences in Germany. This improvement was greatly facilitated by the changes which the Baconian system of philosophy introduced into the mode of cultivating the natural sciences. The author traces satisfactorily its consequences, in the rapid abandonment of many old superstitions relative to the action of poisons and treatment of poisoning, such as the utility of Bezoar, and of the philosopher's stone in counteracting their effects on the body,—the existence of remedies which could draw poisons from internal organs to the external surface of the body,—and the supposed virtues of potions for exciting love or procuring abortion.

We now arrive with Dr Marx at the commencement of the second part of his volume, where he sets out with the history of toxicology from the middle of last century. From this time onward its progress, like that of the other natural sciences, advanced with a rapidity previously unparalleled. The true spirit of the inductive philosophy had infused itself more and more into the inquiries of its votaries. Observation and experiment were habitually resorted to, instead of hypothetical reasoning and reliance on ancient dogmas. The systematizing of the sciences of botany, mineralogy, zoology, chemistry, pharmacy, therapeutics, and pathology had an incalculable influence on the soundness of the views of physicians as to the physiological action of poisons, the phenomena developed by them in man, their effects as therapeutic agents, the mode of discovering them, and their general medico-legal relations.

In following out its brilliant career after the middle of last century, the author commences with a historical notice of the chief researches made on the Physiological Action of Poisons; in the course of which he gives a careful analysis of the experimental investigations conducted on this point by Browne-Langrish, Sproegel, Viborg, Doltz, Vassali-Eandi, Brodie, Emmert, Mayer, Orfila, Schubarth, Gmelin, and some others of less note. He then enters generally into the toxicological method of inquiry by experiments on the lower animals; mentions the illustrations derived through its means of the phenomena produced by animal poisons, such as the bite of the viper, fish-poison, and rabies; gives a particular account of the revival by Fontana, Viborg, Fischer, Hertwich, Nysten, Magendie, Gaspard, Coindet, and Orfila of the ancient experiment of injecting poisons into the veins; enumerates the attempts made by various physiologists to apply electricity in examining the action of poisons, in counteracting their operation on man, and in detecting their presence; then quotes the experiments of Jaeger, Schabel, Marcet, Macaire-Prinsep, Schubert and Zeller, Turner and Christison, and some others as to the effects of poisons on plants; next enters into considerable detail in stating the sentiments and observations of authors relative to the circumstances by which the operation of poisons is liable to be modified—such as soil, climate, season, the time they have been kept, the mode of preparing them, the form in which they are applied, the age of the individual, the quantity of the poison, habit, the kind and quantity of the medium of administration, the swallowing of certain substances recently before or soon after the poison was taken, constitution, temperament, idosyncrasy, manner of life, pre-existing or contemporaneous influences, the state of corporeal and mental health, the quarter where the poison is applied, external and internal poisoning, through the skin, respiratory organs, alimentary canal, vagina, rectum; and he concludes his physiological sketch with a relation of the facts collected in illustration of the question, how poisons act on the body and occasion death? In the last section he considers what has been done to discover the poisons which have a local action, the cause of the spasmodic symptoms excited by many poisons, the entrance of poisons into the blood, the changes produced by them in the mass of the blood, the excretion of them from the blood, and other fluids and solids of the body, the cause of the fluid state of the blood produced by many poisons, the relation which rapidity of decay bears to the action of poisons, the absorption of them by the lymphatics, and the question, how far in death from poison the fatal effect is occasioned through the medium of an operation on the lungs, the heart, the mucous membranes, the nerves, the brain, and the spine. The section on the physiological action of poisons is then closed with an appendix on the experiments made by a few toxicologists on the human subject, and another on the opinions expressed by writers on the practice of medicine as to the operation of poisons.

The remaining sections of the work, and the most important of all, are seven in number, namely, on the general literature of toxicology; on the qualities of some substances whose poisonous nature is doubtful; on the diagnosis and prognosis of poisoning; on the general treatment of poisoning; on the employment of poisons in the practice of physic; on the medical jurisprudence of poisons; and on poisons in relation to medical police.

In the first of these sections he enumerates all the European writers on general toxicology from the middle of last century down to the present time, and gives a useful view of the method and contents of the works of each. In the second he mentions what various authors have said of the deleterious properties of bull's blood, the blood of other animals, the menstrual fluid, and warm blood; then alludes to the difficulty of following what the ancients have written of particular poisons owing to the impossibility of now identifying them by the ancient names; and adds a collection of facts on slow poisoning, on the lead-colic, the mercurial disease, and the secondary effects of poisons generally. Under the section on the diagnosis of poisoning he brings forward all that has been written on the accidents and diseases which are apt to be mistaken for poisoning, and enumerates the following list—the incautious drinking of very cold, or too strongly spirituous fluids, colic, inflammation of the stomach or intestines, cholera, derangement of the secretion and excretion of bile, and apoplexy, and enforces, by the opinions of various authors, the necessity of great caution in pronouncing on the occurrence of poisoning, on account of the closeness with which these diseases imitate it. A similar catalogue is then annexed of the morbid appearances which imitate the effects of poisoning, such as vascularity of the stomach, stains, dissolution, softening, and perforation of that organ. This inquiry is followed by another section containing various historical notices of the progress made in the general treatment of poisoning since the middle of last century, the opinions of authors on the virtue of chemical antidotes, the treatment of the bite and sting of venomous animals, the empiric treatment generally of poisoning, especially of the effects of the gases which prove quickly fatal, the extraction of poisons from the stomach, the use of antidotes which envelope the particles of poison, the employment of artificial inflation, certain methods for restoring and regulating the circulation, some specific antidotes, the treatment of the subsequent effects of poisons, and the moral management of cases of poisoning. The next relates to the views of modern authors on the employment of poisons in the practice of physic; and under this head, an attempt is made to appreciate the share which modern chemistry has had in extending the materia medica by the addition of new remedies from the class of poisons, to lay down the general indications for the medicinal use of poisons, and to enumerate the poisons on whose medicinal virtues satisfactory observations and experiments have already been made. This section concludes with a few remarks on the abuse of poisonous drugs, especially of the preparations of mercury and arsenic.

The succeeding section, on the contributions made since the middle of last century to the medical jurisprudence of poisons, is necessarily a long and interesting one. It comprehends a notice of the works of the chief modern writers on medico-legal toxicology in the different continental kingdoms, and contains a discussion of the following points:—the relative degree of mortality of poisoning;—the possibility of confounding poisoning with other occurrences;—accidental and intentional poisoning;—the collateral circumstances to be attended to in cases of poisoning;—in what cases there is a certainty, and in what others a probability only of poisoning;—the difficulties in the way of deciding on the occurrence of poisoning;—the necessity of attending to concomitant circumstances and pre-existing symptoms;—of performing a careful examination of the dead body, and of discovering the quantity of poison administered;—the precautions which must be observed in the chemical investigation and in the medico-legal inspection of the body;—the utility of disinterment for the purpose of making an inspection;—the limited utility of experiments on animals;—by what persons, and in what manner the chemical inquiry is to be conducted, with examples of the insufficiency of the earlier chemical analyses;—general rules for analyzing suspected substances;—the necessary reagents;—the importance of separating the actual poison when possible;—and of conducting comparative experiments on what is known to be poison.

The last section presents a similar view of modern authors on the medical police of poisons. The subjects under this head to which the author's attention has been chiefly turned are—the precautions necessary to secure children and others against poisonous plants growing wild, or in gardens, more especially certain German municipal regulations in regard to the deadly night shade, water-hemlock, and meadow saffron; lists of the poisonous vegetables of Germany; regulations as to the sale of mushrooms; poisonous honey, and the intentional poisoning of flowers; a chronological account of the statutes of various countries as to the sale, packing, and transmission of poisonous substances; proposals as to a convenient poison for destructive animals; the necessary precautions in the sale of victuals, more especially against spurred rye and spoiled hams, cheese, and sausages; the adulteration of wine, brandy, beer, tea, milk, butter, cheese, vinegar, oil, salt, sugar, and tobacco; on poisonous colours; the danger of certain culinary vessels, particularly of lead, copper, tin, brass, and zinc; the deleterious effects of various trades where poisons are habitually handled or breathed, and the safeguards which have been discovered for the workmen.

We have thus given little more than an index of the vast undertaking of *Dr Marx*. It is impossible to do more for his work in moderate compass; but, at the same time, we must allow, that it is scarcely possible to convey an adequate idea by such an abstract of the amazing mass of erudition which he has collected in his volume. If the forthcoming volume, in which he is to treat the subject systematically, shall be as perfect in its line as that already published, the science of toxicology cannot fail of being a great gainer in many ways by the author's labours.

ART. V.—*Surgical Observations on the more important Diseases of the Mucous Canals of the Body; being a second edition of the Author's Treatise on Stricture of the Urethra.*

To which are added Practical Observations on Contraction of the Œsophagus and Rectum; an Essay on the diagnosis of Hernial and other Tumours in the Groin; with Remarks on Tracheotomy, as connected with the Treatment of Chronic Laryngitis. By GEORGE MACILWAIN, Member of the R. Col. Surg., Lond.; &c. London, 1830, 8vo. pp. 337.

THE frequency of publications on the subject of stricture is an indication of two circumstances at least;—that the disease is very prevalent, and that its treatment is neither very successful nor very well defined. Both circumstances are probably to be ascribed to the same general cause, that stricture, like other diseases, assumes different forms, and that the treatment which is suitable to one variety of the disease is not adapted to the other. First comes Sir Everard Home, the advocate of nitrate of silver and the armed bougie, yet occasionally using the solid steel sound; [*] then comes Mr Whately with caustic potass, yet condemning the nitrate of silver; [†] Mr Luxmoore follows, and discovering that both are bad, and that the bougie, if well-managed, is the most effectual, tells the surgeon he must use first the plaster bougie, and then the metallic one and silver catheter; [‡] then follows Mr Charles Bell, with his urethra sound or ball-probe, the silver catheter, the sound, or the bougie without caustic; [§] then follows Mr Arnott, with the dilating bougie; [‖] another advocate for lunar caustic appears in Mr Andrews; [¶] the same plan is defended by Mr Hammick when the simple bougie is found inadequate to the removal of the obstruction; [**] and lastly, Dr M'Ghie [††] and Mr Stafford [‡‡] come and divide the strictured portion of the urethra by a cutting instrument. Which of these methods is the best, the surgeon naturally asks? Which is the safest and most effectual?

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[*] Practical Observations on the Treatment of Strictures of the Urethra, and in the Œsophagus. By Everard Home, Esq. F. R. S., Surgeon to St George's Hospital. 3d Edition, London, 1805. Vol. ii. 1803. Vol. iii. London, 1821.

[†] Observations on Mr Home's Treatment of Strictures in the Urethra, &c. By Thomas Whately. London, 1801. And improved method of treating Strictures in the Urethra. London, 1804.

[‡] Practical Observations on Strictures of the Urethra, &c. By Thomas Luxmoore. London, 1809.

[§] Letters concerning the Diseases of the Urethra. By Charles Bell, London, 1810.

[‖] A Treatise on Stricture of the Urethra, containing an account of improved methods of treatment, &c. By James Arnott of the R. C. S. London. 1819.

[¶] Practical Observations on the application of Lunar Caustic to Strictures in the Urethra, illustrated by Cases, &c. By M. W. Andrews, M. of the R. C. S. 1st Edit. 1807. 2d Edit. London, 1827.

[**] Practical Remarks on Amputations, Fractures, and Strictures of the Urethra. London, 1830.

[††] Edinburgh Med. and Surg. Journal, Vol. xix. p. 361.

[‡‡] A series of Observations on Strictures of the Urethra, &c. &c. By R. A. Stafford, London, 1829. Farther Observations, &c. and Ed. Journal, Vol. xxxii. p. 139.

In our account of the method of the latter gentleman, we endeavoured to show that all strictures are not of the same nature, and consequently, that all do not admit of a uniform method of treatment. The justice of this view had also been recognized by the present author, who remarks that, while surgeons have generally recommended one kind of treatment to the exclusion of others, the variety in the nature of strictures does not admit of this exclusive mode of practice. The inconvenience of this system of uniformity, Mr Macilwain informs us, he soon began to experience, and he was thus led, by attentive observation, to distinguish the different classes of stricture in which each mode was peculiarly applicable. More extended experience enabled him at length to establish what he conceived to be certain fixed rules in this department of surgical practice; and the explanation of these rules is the object of the present treatise.

Such may be said to be the plea on which Mr Macilwain justifies the publication of another work on stricture of the urethra. We are never displeased with the appearance of a new treatise, however trifling be the excuse for publication; and we do not deny that in the present instance the undertaking is laudable, if not requisite. There can be no worse symptom either of a science or of an art than total stagnation; and if there is nothing absolutely new in the present work, there is at least a degree of merit in attempting to give greater precision to the mode of treatment, and to accommodate it more accurately than heretofore to the varieties of contracted or otherwise diseased urethra. It is not our intention, therefore, to mortify Mr Macilwain, when we tell him that twenty years ago the same task was undertaken by Mr Charles Bell. To come after such a model can be no discredit; and the best reason for the appearance of such a work as the present, is, that if Mr Bell himself were to republish his own treatise, it would certainly derive some benefit from the general experience of twenty years.

The author begins his work with general observations on stricture, in which he considers the situation, form, and extent of this morbid state of the urethral membrane, the age at which it is most likely to occur, and the causes concerned in its production. In these observations there is nothing particularly new. He thinks, however, that stricture proceeds, not so commonly from inflammation, as "a sort of irritation, moderate, but not continued, and distinguished principally by morbid increase of sensibility, sometimes attended by discharge." This hypothesis has a degree of fashion to recommend it; but it would not be difficult to show, that what is named irritation is only a stage of inflammation, and that, on the whole, the distinction is trivial and unimportant. On this ground, however, the author regards injections as rather preventing than inducing stricture, since he infers, by removing the irritation and discharge, they consquently remove the cause on which the formation of stricture depends. He, nevertheless, establishes four very judicious rules regarding the use of these applications in the treatment of gonorrhœa:—1*st*, That they should never be used while there is pain in micturition; 2*dly*, that they should never be of such a quality as to produce more than temporary smarting; 3*dly*, That their strength should be increased as the canal becomes less susceptible; and, 4*th* and lastly, That their action should be confined to the seat of disease.

The next subject which Mr Macilwain explains, is what is denominated irritable urethra, and which he represents to be a morbid sensibility of the urethral membrane, affecting either the whole canal or one portion, most frequently the prostatic. The effect of this is to render the stream very variable, sometimes large, at other times unusually small,—occasionally a feeling of strangury, and sometimes involuntary emissions.

After two chapters on the form and use of instruments, and on the symptoms of stricture, the author proceeds with what may be regarded as the immediate subject of his treatise,—the treatment of strictures. He begins with remarking, that, though the principles on which the restoration of the canal to its natural constitution has been attempted, have varied considerably, the general object has been, either to effect a mechanical but gradual dilatation of the contracted portion,—to remove impediments by the application of substances, the precise nature of whose action is unknown,—or to destroy them by the application of caustic. In attempting the removal of stricture by the first method, Mr Macilwain is averse to the use of the dilator, and decidedly prefers that by the successive introduction of bougies and instruments of increasing diameter. His reasons against the plan by the dilator may be understood from the following extract.

> "A very little consideration of those changes which constitute disease, or those by which organs are restored to their healthy condition, will convince us that neither one or the other can be effected suddenly; and that the safety with which the latter is effected, is, generally, more or less, in proportion as its accomplishment is gradual. This remark is equally applicable to the diseases of other parts, as it is to those of the urethra. It is not consonant with reason to suppose, that we can stretch living parts

as we can inanimate substances, possessing an elastic property; yet we must think after this manner if we have recourse to the dilators; although, as before observed, I have not been in the habit of using them, I have frequently seen the disadvantage of increasing the size of the instrument too rapidly. They never can be of any use except where the stricture is capable of admitting an instrument, however small; and, in such a case, the restoration of the canal can be effected by other means which enable us to regulate the progress of the dilatation with mathematical accuracy. I would beg also to observe, that doubts may justly be entertained whether, by the latter plan, the stricture is removed by dilatation, strictly so called. Many canals of the body certainly, in their healthy condition, admit of this dilatation, and so may the urethra when strictured, provided the alteration in structure be inconsiderable, since the natural texture of the lining membrane does not seem prohibitory of it. But where those changes have occurred, which uniformly attend cases of any considerable duration and severity, it is more probable that the pressure of the instrument produces absorption, either ulcerative or otherwise, of the newly-formed substance; I am, however, of opinion that the dilator is an instrument always unnecessary for the removal of stricture, and frequently improper. For the foregoing reasons a bougie or silver catheter is to be preferred, where it is the object to remove the contraction by dilatation; to which may be added another, viz. the facility with which, in most cases, this is effected." Page 54–56.

Though thus favourable to the ordinary silver catheter or steel sound, however, Mr Macilwain condemns in the strongest terms those with conical extremities, as liable to deceive the surgeon regarding the direction in which he employs force, and very likely to form a false passage. The wax-cloth bougie he regards as inadequate to the dilatation of a contracted urethra, and of no use, unless to take the impression of the stricture. The cat-gut bougie he equally proscribes, as an instrument over which the surgeon has no control,—a sentiment which is not altogether well founded. This exclusion, indeed, of soft, flexible instruments is by no means justified by general experience; and we can assure Mr Macilwain that there are cases in which even the cat-gut bougie passes through a stricture which is totally impermeable to more solid, and, what he may denominate, more manageable instruments. There is, in particular, a species of small wiry stricture, resulting generally from the intemperate use of liquor, or exposure to cold, in which every large firm instrument produces acute pain, and most frequently hemorrhage; and in this the small cat-gut bougie or a small elastic gum catheter may be introduced with almost no force whatever, when the introduction of every other instrument is impracticable. It must be admitted, the introduction of a bougie or catheter, in such circumstances, is not done with the view of curing the stricture, so much as getting into the bladder and educting the urine. But it may be remarked, that the instrument which may be introduced in such a state of the urethra ought not to be rashly banished from the treatment of stricture.

Among metallic instruments, Mr Macilwain properly condemns the flexible, and among the inflexible, he justly gives the preference to the common silver catheter. "It admits,' he remarks, "of an exceedingly fine polish and uniformity of surface, which very much contributes to the facility of introduction; its lightness enables us to judge with the greatest accuracy of the force which the obstacle opposes to its progress, as well as that which we may employ to overcome it; and farther, we are assured of its entrance into the bladder by the urine escaping through it."

He condemns strongly the use of nitrate of silver, as a caustic the action of which it is difficult to confine, and as an agent the use of which frequently occasions hemorrhage, retention of urine, and intermittent fever. Yet it is somewhat singular that he commends much the caustic potass as used by Mr Whately, —an agent certainly much more diffusive in its action. We must indeed express our surprise that Mr Macilwain could ascribe to the nitrate of silver, or indeed to any mode of treatment, such accidents as urethral hemorrhage, suppression of urine, and intermittent fever, since it is well known that these effects occur under all modes of treatment, and, however they may be modified in different individuals, and by different external circumstances, are doubtless to be regarded as the genuine and legitimate effects of the urethral stricture. And regarding the caustic potass, notwithstanding the strong and unqualified commendations of that substance by Mr Whately, and the not less friendly testimony of the present author, we must be permitted to doubt not only its innocence, but its fitness in any mode of application. We know from positive comparative experiment, that it is a powerful caustic, and quickly converts into a dead slough a portion of the animal body, much more considerable in extent than that to which it is directly applied. The cauterizing powers of the nitrate of silver we doubt entirely, and have reason to believe, from many experiments made with it, that it operates merely in coagulating the albumen of the fluid or solid parts to which it is applied.

The treatment of stricture Mr Macilwain distinguishes into that of ordinary cases, that those in which caustic potass is to be employed, those in which nitrate of silver is preferable, the treatment of the long stricture, and of those in which incision is required.

In the first class of cases after ascertaining the state of the urethra by the elastic gum catheter, or the common wax-bougie, the former preferably, he introduces a silver catheter of the size indicated, if possible, quite through the strictured portion, and leaves it in the bladder a short time only. The dimensions of the instrument are subsequently enlarged at each successive introduction. When, from the firm and unyielding character of the stricture, it is impracticable to augment the size of the catheter, he employs the caustic potass according to the method of Mr Whately; and one or two applications of this remedy he represents as in general sufficient to enable the urethra to admit an instrument one or two sizes larger than that, the introduction of which, previous to the application of the potass, was impracticable. After this has been effected, the original plan of introducing instruments of successively increasing size must be continued. In recommending the perseverance in the use of the instrument till the canal admits one of full size, he does not attempt to define the dimensions, but allows that they may vary for each individual. No. 14 of the ordinary scale, however, he thinks may be sufficient for most persons. And, as the disease is liable to recur, he recommends that the urethra be examined occasionally afterwards. The directions on the auxiliary, local, and constitutional treatment deserve the attention of surgeons who are in general too much disposed to rely on mere mechanical measures for the cure of urethral diseases.

The cases in which Mr Macilwain represents the caustic potass to be indicated are described in the following manner.

"In the first place, I would observe, that in those individuals who have been presented to my observation, there has been some very obvious indication of a disordered state of the general health; although the extent of this, as well as its particular feature, has varied considerably. The usual symptoms of stricture are present, and, in general, severe; the peculiarly distinguishing character, however, is a most remarkable sensibility of the stricture. However gently you may pass an instrument,—however soft, or, indeed, whatever be its composition, the moment it arrives at the narrowed portion of the canal, the patient complains of severe pain; and, in those cases which I have seen, blood also invariably follows it when withdrawn. It is this latter circumstance which is calculated to lead the practitioner into error; mere irritability so commonly attends a stricture, and is so frequently relieved by the kali purum, that the excessive degree of it which characterizes this case would not alone, in all probability, prevent a surgeon from at once applying this caustic. But a general impression, and so far not an erroneous one, prevails, that if a stricture bleeds, it is more prudent to delay the use of caustic, even where it is proposed eventually to employ it. I was much perplexed by these cases when I first met with them. I used various kinds of instruments;

every measure that was calculated to relieve general or local irritation was strictly enjoined, and, as I had reason to believe, implicitly adhered to. Still, however, at the subsequent visit, the same failure in introducing any instrument occurred; and, notwithstanding the gentlest manipulations only were employed, the pain was still considerable, and blood (in some cases to the extent of several ounces) followed after the instrument was withdrawn. Thinking, however, that these were only extreme cases of irritable stricture, and that the ungovernable disposition to hæmorrhage was merely the result of the extraordinary sensibility; I applied the kali one morning to a stricture which was at the time bleeding pretty freely, although the blood did not, in fact, flow otherwise than guttatim. The pain during its application was very considerable; yet, as it was not more than that which had been endured from the contact of the unarmed instrument, no part of it was, I think, attributable to the kali. The relief which followed, even within the space of twenty-four hours, was very considerable. Every symptom became diminished in severity, and the painful sensibility completely subsided after two applications; the disposition to hæmorrhage also no longer existed. I have since repeated this practice in every case in which these circumstances were present at the commencement of the treatment, without any hesitation, and have never had reason to regret so doing; the same good effects have been invariably the result. In all cases, the whole of the plan calculated to relieve irritation, has been perseveringly adopted at the same time, therefore I cannot say what would happen if the kali were trusted to alone: were the surgeon to neglect this part of the treatment, it would be a gratuitous relinquishment of measures well known to be beneficial; so that it is scarcely necessary for me to insist on the expediency of their adoption. The cases to which I have alluded, were those where at the same time no instrument, however small, could be passed into the bladder; a circumstance, which the subsequent progress showed to depend, in great part, on the very small opening through the stricture."—Pp. 88–91.

These statements, which are given with a great appearance both of candour and accuracy, we are bound in the meantime to admit; and if they prove on future and more extensive trial to be well founded, Mr Macilwain must be allowed to have effected a considerable improvement in the mode of treating urethral stricture. Mr Macilwain does not attempt to give any explanation of the mode in which the caustic operates in changing the character of this irritable and hemorrhagic form of stricture, as it may be denominated; and indeed this is of less moment providing the fact of changing its character to that of inirritability and indisposition to bleed is well established. He makes, indeed, a statement, that the nitrate of silver produces a slough, while the potass only stimulates, which he regards as adequate in some degree to account for the peculiar action of the latter. But this statement, which is directly the reverse of what we have found to be the case on experiment, we cannot adopt. Something we allow must be granted to the small proportion in which Mr Macilwain informs us he uses the potass, and not a little to its combination with the oily matter employed with it. But in the same manner it may be said, the nitrate is used in small quantity, and the albuminous nature of the urethral mucus, as well as the membrane itself, deprives it of much activity by almost instantaneous decomposition.

Partial, however, as Mr Macilwain is to the catheter and the potass, he does not wholly discard the nitrate of silver; and he allows the necessity of employing it in cases in which the contracted part of the canal betrays extreme want of sensibility. This property he admits his inability to define with accuracy, since nothing is more common than to find a stricture with little sensibility in some persons; but it may be partly understood from the fact, that the rudest manipulations ever warrantable are unaccompanied by pain and unfollowed by blood, and that the only disagreeable symptom is the straining, which he represents to be violent, but distressing rather than painful, —a mode of expression which sounds something like a contradiction. He further remarks, that the kind of stricture requiring the application of the nitrate of silver is exactly the reverse of that demanding the use of the potass. In such circumstances he applies it in the usual manner practised by Sir E. Home. Both in those requiring the potass and those demanding the nitrate of silver, Mr Macilwain impresses the necessity of auxiliary, constitutional, and local treatment, according to circumstances.

Though stricture in general consists in a small part of the urethral canal becoming contracted, it happens in some instances that a portion of the canal, half an inch long or more, becomes continuously more or less narrowed. The manner in this variety of stricture, denominated the long, becomes formed, may be either that the intermediate portion between two separate strictures may also become contracted, or that the whole space may have been simultaneous and originally narrowed by the existence of general inflammation. This species of stricture requires peculiar management. The opening of the narrow space may not be central; and if an instrument is passed it is powerfully resisted, either from its point not entering the orifice, or from the rigidity of the part communicating a similar sensation to that resulting from the insensible stricture. If the surgeon at length succeeds in introducing an instrument through such a stricture, he feels that it does not move more or less freely, as in the ordinary case after introduction, but the instrument grates as it were, and moves with difficulty over a considerable space. In withdrawing it in like manner, it is firmly grasped, and, instead of at once escaping, as it were suddenly or with a jerk, it is only to be extracted by a slow and gentle but unremitted force. For this stricture the best remedy, according to the experience of Mr Macilwain, is the silver catheter; and although even this is not always successful, it presents greater advantages than any other method yet proposed. The time during which the instrument is left in the urethra should be lengthened gradually, always allowing for symptoms of irritation, in the case of which it should either be deferred or left a shorter space. Though the author admits that in this variety of stricture the caustics are productive of little advantage, he nevertheless allows that they may be tried when the catheter seems stationary or to make little progress.

In considering the variety of stricture requiring incision, though Mr Macilwain gives a just view of the cases requiring this peculiar method, he does not appear to make any observations of sufficient novelty or interest to require especial notice. It is chiefly the inveterate cartilaginous stricture, we conceive, in which this method is indicated, and that we have considered at sufficient length in our account of the method of Mr Stafford. It is just to add, that the present writer expresses himself in a very candid manner of the method proposed and practised so successfully by Mr Stafford. It is evident that it is no inconsiderable advantage to obviate the necessity of cutting down on the urethra from the surface.

The 11th and 12th chapters, on retention of urine and perinæal abscesses and fistulæ, present a tolerably correct view of the best and most efficient mode of practice in these diseases. The 13th contains some observations on urethral stricture in the female, with a case in which the author, after various unsuccessful attempts, at length dilated the urethra so far as to allow the introduction of a metallic sound, and eventually a fine French catheter.

"On endeavouring to introduce the catheter, the membrane over the meatus was found to be considerably thickened and plicated: and this change was continued a short distance within the vagina; nothing like a distinct meatus could be observed. On passing the instrument between the folds of the membrane formed in the situation of the orifice of the urethra, it entered easily to the extent of an inch; and examination by the finger in the vagina, clearly showed that the catheter was in the urethra. All my endeavours, however, to introduce it into the bladder proved fruitless at this time; and I was therefore obliged to be content with exerting a certain degree of pressure on the stricture. The point of the catheter became so firmly embraced that I was enabled to secure it in this situation, and desired the patient to allow it to remain there, if its presence was at all tolerable. The warm bath, or rather semicupium, was ordered to be repeated. The following day I again visited the patient: I found that she had only been able to endure the presence of the instrument half an hour; but the urine had since flowed more freely, and she was much more comfortable. I now succeeded in passing a small metallic sound without much difficulty, and the irritation it excited was very inconsiderable. My next four visits were repeated every other day; and at each I increased the size of the instrument, with one exception, when I was obliged to be content with introducing one of the same size as had been previously employed; the patient had become quite easy. Before the time appointed for my next seeing her arrived, I was sent for, as she again laboured under retention: she had been half an hour in a semicupium when I arrived; and as the bowels were freely open, I at once attempted and succeeded in the introduction of a fine French catheter; the parts, however, were exceedingly irritable. I should have imagined that this attack of irritation was consequent on the too frequent use of the instrument, had it excited any particular irritation; but I am convinced that this was referable to other causes. I may here observe, that I should not have repeated the instrument so often but in consideration of the comparative small degree of sensibility which exists in the female urethra. The following day I found her quite comfortable, and advised her to avoid all sources of excitement, particularly that on which I conceived the present attack of irritation to depend. From this period the case went on favourably; the stream of urine gradually acquired its usual volume, and the frequency of micturition no longer existed; the size of the catheter was increased every second or third day, until a full-sized instrument entered the bladder with facility, when she felt herself perfectly well. This is now more than a year since, and I understand she has had no return of the complaint."—Pp. 163–165.

On the subject of stricture of the rectum, the author gives a fair general view of the etiology, symptoms, and treatment of the disease, and he impresses particularly its connection with constitutional causes. It contains, however, nothing new; and the summary of morbid appearances is given in a manner rather vague and indiscriminating. The method of treatment is judicious, and shows that the author has paid some attention to the mutual influence of general and local measures. The following directions, as to the use of mechanical dilatation, deserve perusal.

"In using the bougie, the same gentleness as is recommended in stricture of the urethra, should be observed, and the regulation of the size selected, should be guided by the same principle. In the majority of cases, the stricture may be felt by the finger, or is within a few inches of the orifice, so that a straight bougie will answer every purpose; but as in all cases it is desirable to ascertain the condition of the whole bowel, and as its course is curved, I prefer employing a curved instrument. The curvature should be a sort of general one, in which the whole instrument participates, the greatest convexity being described by a line bisecting the instrument, into equal lengths. An ordinary bow, half strung, gives, I think, a sufficiently accurate idea of the shape which I intend to convey, particularly if a very little additional curve be added to the point. I generally introduce the bougie thus formed, with its concavity towards the pelvic side of the bowel, and, as it proceeds, carry its handle a little backwards, which enables the surgeon to prevent the point of the instrument from hitching at the projection of the sactum. The introduction of a bougie into the rectum sometimes produces little or no inconvenience, provided it be done with great gentleness; at others, it is accompanied by pain or aching in the part, which continues for some time after it is withdrawn. Some patients also complain of uneasy sensations in the course of the transverse arch of the colon. With regard to the time which the bougie should be allowed to remain, the best rule is, to regulate it by the feelings of the patient, since, if it excite much pain it will do harm, whilst in the absence of any suffering, the longer it be allowed to remain (in reason) the better. I have seldom allowed more than seven or eight minutes at first, but have usually increased the length of time to a full hour, after two or three introductions. We shall seldom find it practicable to increase the size of the instrument with the same rapidity here, as in affections of the urethra; on the contrary, it will be generally necessary, and almost always expedient, to introduce the same instrument twice or thrice before we employ one of a larger diameter, allowing an interval of two or three days to intervene."—Pp. 217-219.

"Sometimes the stricture may be felt by the finger, and if its whole length can be judged of, and the accompanying symptoms are fairly explained by the degree of obstruction which it offers, its division may be safely performed, and the patient happily relieved in a much shorter time than by any other method. The mode of accomplishing this is by the introduction of a common probe-pointed bistoury, or that recommended by Sir Astley Cooper for strangulated hernia. I have always employed the former. It should be introduced lying flat on the finger, against which it should be firmly pressed; this will avoid the risk of its injuring any parts inferior to the stricture. When the finger has reached the contraction, the edge of the bistoury should be turned towards the side on which we propose to divide, the instrument being kept steady by the finger on which it has been introduced. As the hæmorrhoidal vessels frequently become enlarged under circumstances of continued irritation, it is better to divide the stricture by two or three very small incisions in different directions, than to accomplish its division in one. If the latter mode were preferred, a sacro-lateral direction would probably be the most safe; but as the practice above recommended will always effect the desired object, it should be preferred. As soon as the aperture be sufficiently enlarged to freely admit the finger, the case may then be treated by the bougie in the ordinary manner."—Pp. 220–221.

On this point, in general, we must be allowed to recommend the study of the observations of Mr Houston on the rectal duplicatures, in the fifth volume of the Dublin Reports, or at p. 147 of our present number, and a good paper by Dr Colles, on diseases of the rectum in the same volume.

The observations on stricture of the œsophagus contain judicious directions for the management of that disease, and a case illustrating the success of the mode of treatment.

Those on the *diagnosis of tumours occurring at, or in the vicinity of, the groin*, deserve the particular attention of the surgical practitioner. It is true the hospital surgeon cannot derive much advantage, or learn any thing which he does not already know. But to the young surgeon, as yet little familiar with the peculiar appearances of hernia, and diseases with which it is liable to be confounded, these observations may be of some use.

In speaking of tracheotomy, he justly cautions against the division of the thyroid veins, and notices a distribution of the *innominata* and right carotid, which might be very inconvenient.

> "The thyroideal veins will frequently be in the way of the operator, and will sometimes cross the wound exactly at the point in which it is proposed to open the tube, and these veins, if there be any accompanying affection of the thyroid gland, will be considerably enlarged. In a case which I shall mention, a very considerable venous trunk crossed the trachea in this manner, which was pushed on one side by the handle of the scalpel. The division of these veins should be avoided if possible, as the operation has been known to fail in consequence of the blood issuing from them falling into the trachea, and suffocating the patient. This might certainly be prevented by tying the vein which had been wounded; but the irritation which is sometimes consequent on the ligature of veins in other parts, renders it very desirable to avoid the necessity of such a measure. When the integuments are fairly divided, the operator seldom requires any other part of his knife than its handle until he arrives at the trachea, in which he should make a longitudinal slit correctly in its front aspect, large enough to admit a tube of the size figured in the plate. The shoulder of the tube should be padded by a few folds of lint, and secured in its situation by tapes attached to the apertures, and fastened again behind the neck. This opening will answer every purpose in general; but if the circumstances were such that no convenient tube were at hand, it might be necessary to remove a portion of one or two of the cartilaginous rings, which has sometimes been done. It should never be forgotten, that the arteria innominata sometimes rises so high in

the neck before its division, as to cross the trachea in the situation where the tube is opened in this operation. A similar distribution has also been observed with regard to the right carotid. Of the first named variety I have seen three examples, and to show the necessity of bearing these facts in our recollection, one of them occurred in a patient on whom it was proposed to perform tracheotomy."—Pp. 328-330.

For the former we refer to the Medical Intelligence of the present number.

Art. VI.—*Transactions of the Medical and Physical Society of Calcutta*, Vol. IV. Calcutta, 1829. Pp. 449. 8vo.

We have already on several occasions noticed with approbation the proceedings of the Medical and Physical Society of Calcutta; and in introducing the fourth volume of its Transactions to the reader's attention, are happy to have another opportunity of bearing public testimony to the zeal with which its members continue to work together for the promotion of professional knowledge. As the Society now consists of upwards of 280 members, its ramifications extend through every corner of at least the Bengal Presidency, and it must consequently command the means of investigating every medical topic of importance in that interesting but, in many professional respects, still unknown country. In consequence the present volume contains communications on every variety of professional subjects, and from almost every quarter of our Indian possessions; and of these communications there are very few which will not be perused with interest by the European reader, as well as by those for whose instruction they are more particularly intended.

The volume contains thirty-three papers, and eleven articles of an appendix. Many of these papers and appendixes consist simply of narratives of remarkable cases; but there is likewise a fair proportion of more elaborate essays, among which those on medical topography are the most numerous and most novel in their nature. It is of course out of the question to attempt to transfer even any material part of this great mass of information to our pages. We shall best consult the leading objects of the present review,—namely, to give publicity to the Society's labours, and to communicate to our readers so much interesting intelligence as may induce them to refer to the original,—by giving a short analysis of what we consider the most important articles.

Commencing with the department of Medical Topography and Statistics,—a deeply important topic in every country, but nowhere more than in India, where so many sources of inordinate unhealthiness are open to investigation,—we find a paper on *the General and Medical Topography of the Neelgherry Mountains*, by Mr D. S. Young of the Madras Establishment; *Observations on the Medical Topography of Tirhoot*, by Mr J. Evans, Surgeon in the Bengal Establishment; on *the Climate of the Jyntea Hills*, by Dr Beadon; on *the Climate of the Deyrah Dhoon*, by the Honourable Mr Shore; on *the Climate of Mullye*, by Mr J. Tytler; on *the Climate of Pooree*, by Mr Brander; and *on Public Health in India*, by. Dr D. Butter.

The greater number of these essays relate to the topography of districts in India, chiefly of recent discovery, where the usual causes of unhealthiness in the East are in a great measure wanting; so that the European may not only enjoy there a state of health equal to that of his native country, but likewise, after suffering from the diseases of Hindostan, may recruit his dilapidated constitution, without the expensive measure of paying a visit to Europe. We need scarcely say, that if the institution of the Medical Society of Culcutta shall have contributed towards so invaluable a discovery, the utility of its foundation will be fully shown, and every possible inducement held out to its members to continue their exertions.

The paper of Mr Young on the Medical Topography of the Neelgherries is one of the most interesting, perhaps the most important, in the whole volume. It is an essay of upwards of forty pages, on the climate, geological structure, inhabitants, general resources, and healthiness of a mountainous district in the Mysore territory, about thirty-six miles long and fifteen broad; which, if recent accounts are to be trusted, is as much exempted from the scorching heats, and deluges, and diseases, as it is elevated above the level, of the surrounding plains. Its peculiarities appear to have been but recently brought under the notice of the Eastern public. "It was not until lately," says Mr Young, "that the Neelgherries were explored by a few philanthropic individuals; who, in the enthusiasm created by the grand discovery they had made, gave to the world a faithful picture of what they had seen; and, though far less vivid in its colouring than the scene of magnificence which their wondering eyes beheld, no credit whatever was attached to it. People were taken by surprise. Some entirely disbelieved that such a country and climate could exist within twelve degrees of the Equator; others, who understood the principle on which the temperature of elevated regions is regulated, could not conceive how the Neelgherries, so long within the limits of the Company's possessions, should have hitherto escaped notice; and consequently presumed that their elevation was far less than it was described to be: and but for the few disinterested persons alluded to, these celebrated mountains might have been consigned to the family vault of all the Capulets."

The highest points of this mountainous country are between 8000 and 9000 feet above the level of the sea; and the villages are generally situated at elevations varying from 3500 to 8000 feet,—the highest of which appear to be the healthiest. The country presents few table lands, but is composed of a succession of undulations, with occasional peaks, presenting the greatest variety of rock and river scenery. The rocks are in general primitive; granite and gneiss form the greater part of the district; greenstone forms the substance of the peaks; and conglomerate rocks appear where the high country meets the table land below. The animals found in it are those of India generally, with the exception of the elephant, which never ascends from the jungle country of the plains. Among the birds are several European species, the blackbird, thrush, tomtit, and woodcock. The plants are many of them peculiar; but many too are identical with those indigenous in Europe.

Of about 470 square miles which compose this territory, only fourteen are under cultivation, the employment of the inhabitants being chiefly pastoral. The natives consist of several castes, among which the Todawurs, or lords of the soil, are the most remarkable. They are all shepherds, "tall, robust, and well-proportioned. Their hair is so thick, that, being kept about six inches long, they require no other covering; and with their fine bushy beards and Roman noses they have quite a venerable, heroic appearance." They are extremely gay, fond of laughter to an excess, of ingenuous disposition, proud of their liberty, which they retain entire, and passionately attached to their native hills, which they are never known to leave. The whole population of the district does not exceed 4000 souls.

The climate of the Neelgherries is one of the most extraordinary in the whole world. The temperature is remarkable for its equability, and equal exemption from extreme heat and intense cold. The thermometric range, from the highest of the hot months to the coldest of the winter months, is less than that of any part of Europe. At Seerloo, on the verge of the district, and 3500 feet high, this range is from 99° to 54°. But at Jackanary, 5000 feet high, it is from 75° to 43°; and at Ootacamund, a village in the centre of the country, elevated to the height of 8500 feet, the range is from 69° to 31°. The mean annual temperature at Jackanary is 60°, at Ootacamund 56½°. In the winter months, when the greatest monthly range occurs, the thermometer has never been observed lower than

28, or higher than 59½° ; except in these months, the monthly range of extremes varies from 2½ to 10° at most; and, during the rainy season, the range is only 2½, the thermometer being for days together so stationary, that some persons have supposed their instruments were out of order. The power of the sun is very low; it varies from 12 to 25° only above the heat in the shade; at Ootacomund, the maximum in the sun is 85°, the minimum 66°. The spring commences at the end of February. "Vegetation is slow and steady, except during the frosts: all Nature's productions, though on the grand scale, are equally removed from a rank and stinted vegetation, and are distinguished by that sober cast which characterises the true medium of healthfulness and beauty." The average fall of rain annually is about 64 inches, or nearly three times that of England. A considerable part of this falls in the shape of refreshing showers during the summer months; for a striking circumstance in the climate of the Neelgherries is, that the temperature very rarely reaches to 68° or 70°, without clouds and showers speedily arising to temper the heat. The air is in general very serene, even when in the surrounding plains it is foggy and obscure. On viewing the table-lands of Mysore from Dodapet, the highest peak of the Neelgherry range, "clouds are seen far below floating above them; and the same phenomenon is observed while looking from Kotygerry towards the plains. They seem to rest on the intermediate zone, while the sky above is perfectly clear and free from vapour."

The information collected by Mr Young with regard to the diseases of this happy district is necessarily limited by the opportunities of a twelvemonth's residence. The lower castes are liable to bowel camplaints; and catarrh is common, though seldom or never dangerous. The fevers of India are unknown. Mr Young feels satisfied, that every case where jungle fever has appeared to originate in the European residents, was distinctly referrible to an elephant hunt, or other temporary absence in the unhealthy plains around. "The spasmodic cholera has never yet ascended the Neelgherries, though its ravages in every part of the surrounding country were of the most appalling description." European invalids, on arriving in the high lands, especially those who have suffered from bilious fever, are liable at first to a troublesome giddiness; which, however, is soon removed by smart purging, and exercise on foot. Bilious diarrhœa is another affection to which such persons are for some time subject, particularly if they have suffered from obstructions of the liver, or habitual constipation; but it is a mild complaint, which Mr Young considers "a sure indication of the sanative powers of the climate, and may therefore be regarded as critical, and the precursor of good health." From an extensive observation on European invalids resorting thither, it appears that there are very few of the chronic sequelæ of Indian diseases, which do not yield to the climate of the Neelgherries in the course of twelve months.

"Affections of the head, whether spontaneous or proceeding from wounds or injuries; obstructions in the liver, with fever; paralytic affections; the languor and debility proceeding from sedentary habits in a hot climate; constipation of the bowels from the same cause; phthisical tendencies; constitutional ulcers of an indolent nature; depravities of the habit; hypochondriasis and bowel complaints,—have all been cured or relieved by a twelvemonth's residence on the hills; and I have observed with astonishment, that many got rid of their complaints though pursuing a course, not only in opposition to the instructions and advice of their medical attendant, but calculated to injure the very best constitutions.

"In the sequelæ of remittent or jungle fevers, I should suppose that a trip to the hills would be better and less expensive than a voyage to the Isle of France or the Cape. This, above all others, is the climate to set up a poor fellow who has escaped from the jaws of death, with a system supersaturated with mercury, and a constant tendency to fever at the lunar periods. In a very few weeks, he will be enabled to discard all sorts of purgatives, and will find the air a sufficient stimulus to his digestive organs."

Invalids who have remained long enough to be convalescent, and those who arrive among the hills in a state of health, experience a striking propensity to bodily exercise, and a great increase in the flow of animal spirits. The Neelgherry parties appear from the author's narrative to be the very personification of gaiety: Even persons habitually grave in deportment acquire the character of the *aborigines*, and frequently give way, like them, to paroxysms of ungovernable mirth.

With these extraordinary recommendations, it is not surprising that Mr Young advises the Indian Government to consider well how this benignant region may be taken advantage of, in respect to the medical police of the vast bodies of men now under their authority. With individuals, it is rapidly becoming the Montpellier of the East. But this is not enough. "The natural capabilities of the district," says the author, "are great and inexhaustible. Every thing that can be required for the use of man is either furnished spontaneously by nature, or could be procured by the industry of man. The woods contain the finest timber for building, and the most costly kinds for furniture of every description. The pastures afford abundance of food for cattle and sheep. All the materials for building are scattered over the country in the greatest profusion, and the finest of our European vegetables already vie in luxuriance with the best productions of the parent country. From the barley grown in the hills, which is very cheap and plentiful, very good beer has been brewed; and if vineyards were formed on the high table lands of Davoyrapatan, on the Mysore side, only a few miles from Ootacamund," *(for the grape does not ripen in the climate of the Neelgherries proper,)* "the grapes might be brought up and made into wine, as the proper temperature could with certainty be commanded during the whole of the process." Mr Young therefore recommends that all regiments arriving from England should be cantoned among the Neelgherries till the aggressions of hostile powers required their presence elsewhere; and that regiments which have suffered from hard service should be sent thither to recruit. By these means he is certain that European troops might be kept in a much better state of efficiency than at present; that numberless lives would be preserved, and many men, who are now pensioned and invalided, might be saved to the service for several years.

The author has annexed to his narrative some useful and indispensable instructions for the conduct of those who visit the Neelgherries. They must be provided, he tells us, with European clothing to meet the cold of winter, and must take a substantial house, which ought to be built as much as possible according to the European fashions, with sufficient means to keep out the blast in the cold season. In particular, however, he advises every one, whether invalid or not, when he arrives at the boundary of the district, at the foot of the mountains, by no means to sleep there, or even at a moderate elevation in the valley-passes which enter them; for in these passes, as has been generally remarked in the East, and experienced by those who have been so unlucky as to remain on their way to this happy land, jungle-fever is extremely common and very virulent. The natives close to the mountains have a singular prejudice to them, and are with great difficulty persuaded to ascend them, many who have done so having returned with fevers caught from the clothing suitable to the plains being quite insufficient to meet the bracing air of the hills. Hence visitors must take care to bargain with their bearers at a considerable distance from the mountains to be carried by the same people to their journey's end. They should make a point not to rest a night at Seerloo, though among the hills, and 3500 feet high, for it is by no means a healthy station, but push on to Jackanary, which is 1500 feet higher, and exempt entirely from jungle fever. An invalid should leave Calcutta or Bombay soon after the rainy season. From the former capital he has a fifteen or twenty days' voyage to Calicut, and from the latter a voyage of six or eight days; and from the landing place four easy night journeys in a palanquin will place him on the heights of the Neelgherries.

Mr Young's paper is, in many respects, the most important that has yet appeared in the Society's Transactions. In describing such a region as the Neelgherries, it is very difficult to avoid the appearance of partiality and of a little propensity to romance. But there is ample evidence of its being a climate of most unusual healthiness, and admirably fitted as a nursing station for Eastern invalids.

The Transactions contain several short notices of other districts of a somewhat similar nature, which have been but lately examined with care, and which appear to present great temptations to the European exhausted by the several plagues of the Hindostan climate. Thus Mr Evans gives a succinct description of the district of Tirhoot, in the province of Behar, near the Nepaul territories, a district, he says, justly extolled throughout India for its climate, fertility, and beautiful scenery. But the information supplied by him is rather meagre, and deals too much in generalities. Among the natives bronchocele is common, and a singular circumstance regarding it is, that, on a small river of the district, it is not uncommon to see a village on each bank and quite contiguous,—"the one with scarcely an individual exempt from that complaint, while in the other the inhabitants are perfectly free from it." Dr Beadon also describes the climate of the Jyntea hills, a mountainous range in the province of Assam, about 4800 feet in height, where the temperature is for the most part from 12 to 15 degrees lower than that of the surrounding plain, and appears very rarely to exceed 76°, and which is now frequently resorted to with success by invalids. The Honourable Mr Shore gives a brief and imperfect account of the district of the Deyrah Dhoon, where government have formed an invalid station, but where the temperature seems liable to the same extreme vicissitudes, and the same violent heats with the rest of India. Lastly, Dr Brander gives a desultory sketch of the climate of Pooree, near Jaggernaut, which has been for some time considered the Monpellier of our eastern possessions. This is a station of which an accurate topographical delineation would be a great acquisition. Dr Brander merely mentions that the temperature ranges throughout the year from 64° to 89° and is subject to very little variation during the twenty-four hours, and adds some instructions to invalids as to the proper season and manner of visiting it.

Mr Tytler's account of the Medical Topography of Mullye is of a very different description. This is a district north of

Tirhoot, which we have already noticed, and on the very confines of the Company's territories towards Nepaul, remarkable for being exposed to all the curses of the climate of Hindostan in their most aggravated degree. We must refer to the original picture drawn by Mr Tytler of its horrors, and merely stimulate the reader's curiosity with the following extract, relative to its miseries in the wet season.

"It was invariably necessary to alter the disposition of my library in the dry and rainy weather. It is no exaggeration to say, books of all kinds become, in the latter season, so swelled with moisture, that a shelf cannot then hold more than three volumes out of the four, that it easily contains in the dry part of the year. Books bound with ill-seasoned, particularly with Hindostanee leather, and still more especially if left to lie neglected on a table for a day or two, become covered with a stratum of white mould, at least an eighth of an inch thick; and an approach to this takes place even with the best European leather. The boards are then soaked through with moisture, whilst in the hot winds they are parched and rolled up as if held before a fire." * * Small mushrooms grow in every corner that is the least neglected, even in the most frequented rooms: left to themselves they would attain the height of about two inches, with a top rather larger than a shilling."

Next to the paper of Mr Young, those which appear to us to possess the greatest interest are three elaborate articles on practical subjects by Dr Adam, and Mr Twining, the Secretaries of the Society, and by Dr Mouat. Dr Adam's essay is *on the Epidemic Bronchitic Fever of infants and young children, prevalent in Calcutta during the Rains of* 1828. That of Mr Twining relates to *the employment of large doses of Ipecacuanha in Dysentery, without exciting vomiting.* That of Dr Mouat is an account of *the Cholera Morbus in his Majesty's* 14*th Regiment at Berhampore.*

The epidemic bronchitis described by Dr Adam appears to have been a variety of influenza, possessing the singular peculiarity of confining its ravages throughout the whole period of its prevalence in June, July, and August, to children under the age of four years. Among European children under that age the author thinks that scarcely one in a hundred escaped its visitation; and in every instance it was a formidable disease. Its approach was generally gradual, but its onset so violent that commonly the child was considered in danger at the first visit of the physician. After a few days of slight cough, fever on a sudden set in with violence, while the breathing quickly became much oppressed and stridulous, as in the advanced stage of asthma, and the cough more frequent and accompanied with expressions of pain. Fatal cases occurred only among children under two years, and chiefly indeed among infants at the breast. In such cases death sometimes took place within twenty hours after the physician's first visit. The treatment consisted in topical bleeding by leeches, and antimonial emetics, followed by purgatives of emetic tartar and calomel combined, occasionally by blisters, and very generally by warm bathing. Dr Adam had but one opportunity of examining the body after death. He found the bronchial tubes free of redness, but discharging a thin mucus mixed with a semipurulent secretion, and the substance of the lungs much infiltered with serum.

Dr Adam does not consider that the disease was infectious. While so many children of European parents suffered, the native children were wholly exempt from it. The proportional mortality must have been considerable; for during the months of June, July, and August for the three previous years, the average annual deaths under four years recorded in the obituary list of the Government Gazette was only 21, while in 1828 it was 59.

Dr Mouat's paper is a valuable communication on an epidemic of cholera which attacked the 14th King's Infantry, soon after terminating a march from Calcutta to Berhampore. The narrative presents the same accumulation of extraordinary facts, which other surgeons of the East have recorded of this incomprehensible disorder. The regiment, after seventeen days' march, arrived in their cantonments on the 11th February, 850 strong, and with only thirty-nine men in hospital. On the 14th March the first case of spasmodic cholera occurred, and during the next sixteen days, ninety-three more were attacked, of whom twenty died. The disease attacked chiefly men debilitated by long residence in Hindostan and habits of intemperance. The only peculiarity presented by this, compared with other epidemics of cholera witnessed by Dr Mouat, —but a peculiarity well deserving the attention of eastern practitioners,—was that venesection, which he had previously found the speediest and most efficacious remedy, on this occasion "destroyed the powers of life, sunk the strength, and induced spasms and cold sweats," so that he was obliged to abandon it. When the pulse was hard, the face flushed, and the spasms strong, which seldom happened, it did good; in all other cases it was injurious. Scruple doses of calomel with opium to allay the vomiting, large blisters on the abdomen and spine, ether, ammonia, and camphor, as internal stimulants, hot bottles and friction as external stimulants, and anodyne injections to check the purging, constituted the most successful treatment. The fatal cases were inspected, but in many instances very little disease could be discovered;—in some congestion and vascularity, in others none, in all the same fluid in the alimentary canal with what was ejected during life.

Dr Mouat found it quite impossible to trace the cause of this fatal visitation. The disease appeared about the same time among the privates of H. M.'s 14th and 47th regiments, the native servants, and the inhabitants of the bazar, while the officers of these corps, the civil residents, and the sepoys, assembled in the same cantonments, and mixing freely with the soldiers of the sick regiments, remained free. None of the native attendants at the hospital were attacked; and of 150 patients in the hospital for other diseases one only was taken ill. Dr Mouat, therefore, rejects the idea of contagion being the cause, and indeed seems to despair altogether of its source being ever discovered.

"I have now witnessed," says he, "the attacks of this oriental scourge for upwards of ten years, and though I have studied it, watched its invasions, contemplated its progress, endeavoured to trace its causes, as well as to alleviate or mitigate its symptoms, yet I find it still the same inscrutable, inexplicable, and intractable disease, as when I first arrived in 1817 in this country."—"No laws by which contagious or epidemic diseases are usually guided can explain its anomalies. We shall have all variations of temperature, every age, constitution and habit in a European corps, every species of irregularity and excess, with exposure, &c. and yet no cases of cholera among individuals thus situated. When lo! suddenly and unexpectedly the disease raises its hydra head, creates alarm and consternation; but 'like the baseless fabric of a vision,' it is vain to pursue the phantom. Men are constantly and violently purged for hours, nay days, without cholera: get drunk, and vomit with the most violent retching and straining; still no cholera succeeds. Whereas at another time every purging, every sickness at the stomach, and nearly all the endemics of the season appear converted into cholera."

On the present occasion the men were in the early part of November very healthy. In the five last days of the month, the weather, from being cool, and with steady westerly winds, having become closer and variable, six cases of cholera occurred, of which three proved fatal. In December there was no cholera. In the beginning of January, 15,000 rupees were paid to the men as commutation for clothing; which was followed by the vicious indulgences usual in such circumstances, and by a great increase of general sickness. Among other diseases, seven cases of cholera appeared between the 6th and 19th, of which two were fatal. In the middle of February there were four cases and no deaths. At length, in the middle of March, when the baneful effects of pay-day, the risks of a long march, and the dangers of taking up new quarters, were all passed several weeks before, the disease suddenly appears with virulence, selects a particular corps in the cantonment, and a particular description of members of that corps, of whom, in little more than a fortnight, ninety-four are seized, and twenty die. It is indeed impossible, with our present knowledge, to comprehend its apparent capriciousness. Yet the very specialty of what Dr Mouat terms its *predilections* would appear to render it, in the eye of a European inquirer, not a very difficult matter to arrive at the truth by a strict investigation.

The paper of Mr Twining *on the Employment of large doses of Ipecacuanha in Dysentery, without exciting vomiting*, is an important contribution to the treatment of this formidable disease. The author has applied his practice very extensively, and the result of a dispassionate consideration of his experience is, that he "would most strongly recommend the employment of ipecacuhana in very large doses, for the purpose of relieving tenesmus and irritation, restoring a healthy state of the alvine evacuations, and in more remote stages of the disease promoting the healing of intestinal ulcers." His usual plan was to administer a castor-oil draught in the morning, and at night, twelve hours after the oil, a dose of six grains of ipecacuan with four of extract of gentian and five of blue pill. This was followed next morning by a moderate dose of compound jalap powder; and the same treatment was pursued day after day till the disease ceased. When there was much pyrexia and tenderness of abdomen, with bloody stools, general blood-letting or the application of leeches was premised. And an important adjuvant in the cure was the restriction of the patient to small quantities of food and drink, not exceeding six ounces at a time, and even that allowed but seldom. With these precautions the ipecacuan very rarely excited vomiting, while in a few days, without sweating or any other appreciable physiological effect, the stools assumed a proper feculent appearance, and all the torturing symptoms of dysentery subsided. A considerable number of cases are given by the author to illustrate his mode of treatment. He also shows from various old authors on the diseases of the East, that the practice he recommends has at different times been found eminently serviceable by the physicians of Hindostan.

With these notices, which do not comprehend quite one-half of the matter in the volume, we shall conclude for the present what we have to say of the Medical and Physical Society of Calcutta. Several of the remaining papers will receive some notice in the Medical Intelligence of our subsequent numbers.

This, the fourth volume of the Society's Transactions, together with their previous publications, will amply show that the Society has not been instituted in vain; and we fervently trust that its members will continue to maintain and increase their activity and its reputation. We would especially recommend to their attention the importance of topographical sketches, of which that of the Neelgherries, by Mr Young, in the present work, is a very good model. And still more, for the sake of European, as well as Eastern medicine, we would beg to turn their regard to the various drugs with which the Materia Medica is supplied from Hindostan, and to the numerous plants growing in India which may be found to possess analogous properties with those that yield articles now generally used in medicine.

ART. VII.—*Elements of Surgery.* By ROBERT LISTON, Fellow of the Royal Colleges of Surgeons in London and Edinburgh, Surgeon to the Royal Infirmary, &c. &c. &c. Part First. London, 1831. 8vo. Pp. 318.

THERE are two modes of composing elementary treatises for the instruction of the younger members of the profession. The first consists in taking a general view of the doctrines on the nature of diseases taught by the ablest pathologists, and the modes of treatment pursued by the best practitioners, and selecting from these what the judgment and experience of the author enable him to regard as the most expedient and efficacious. The second consists in the author stating only his own views on the nature of the disorders of which he treats, and giving merely what he himself has found within the range of his own personal observation and experience to be the most effectual treatment, without reference to the observation and experience of other practitioners. Each of these methods, possesses advantages peculiar to itself; and each may no doubt be adopted in a didactic work with benefit to those for whose instruction it is intended. According to the first method the mind of the student is habituated to compare the merits of different doctrines and modes of treatment; and if the treatise is properly composed with a due admixture of candid inquiry and rational deduction, he is gradually trained to that most importcult of all exercises to a professional person,—the application of general rules to particular cases, with such modifications as individual circumstances may demand. A system of surgery composed in this manner, while it exhibits a view of the actual state of the science, indicates at the same time its deficiencies, and endeavours to determine the circumstances on which these may rationally be expected to be supplied.

The character of a work composed according to the second method is of a more limited and exclusive description. Little solicitous about giving the views or hypotheses, it may be, entertained by others on pathological points, somewhat ambiguous and obscure, and equally indifferent about the advantages ascribed by one surgeon to one method, or by another practitioner to another, the author states at once what he conceives to be the true pathology, and what he has actually found to be in his personal experience the most successful mode of treatment. It is manifest, that, in a work composed on this principle, the student is taught positively and precisely one uniform system of theory and practice; and while there is no great room for the exercise either of rational scepticism, inquiry, or the adoption of new principles and methods in circumstances which prove the old to be unavailing, there is equally little for hesitation and indecision.

Every thing in such a system is positively right or wrong, admissible or inadmissible; and there is neither medium nor modification in principle or in practice. There is, however, one objection to this method of instruction, that, whatever be its merits in other respects, it is still the theory and practice of the author only; and though this may accidentally coincide with that of other surgeons, it must always, from the manner of its construction, partake very much of the exclusive system to which he is attached.

Having said thus much of the composition of didactic treatises in general, we proceed to inform our readers, that the present work belongs to the latter class, and consequently participates both in its advantages and inconveniences. It is almost idle to remark, that it ought not to be matter of regret that this is the character of Mr Liston's work. We believe most of those whose judgment in this particular is to be relied on, will agree that the surgical reader must be interested in the appearance of a work exhibiting the principles and practice of a surgeon who has already distinguished himself as one of the boldest and most dexterous operators in this country.

The present volume, which is the commencement only of the work, exhibits a concise view of the general doctrines regarding the principal surgical diseases. The subjects which it embraces are the following. Inflammation, erysipelas, boil, and carbuncle; inflammation as it occurs in mucous, serous, and synovial membranes; inflammation of bone, and its effects, porcelain deposit, fragility, *mollities*, and rickets; inflammation of the arterial system and aneurism; inflammation of veins; tumours, simple, adipose, fibrous, encephaloid, melanoid, carcinomatous, hematoid, the painful tubercle, polypus, encysted tumours, *exostoses*, *osteo-sarcoma*, *spina ventosa*, spongoid tumour of bone, and their treatment; wounds of different kinds; ulcers; hospital gangrene and phagedæna; malignant pustule; ulcers of the genitals, and their consequences; and, lastly, scalds and burns. Of the manner in which the author treats these subjects we shall take a short cursory view.

In considering the pathological state of the blood-vessels during inflammation, Mr Liston espouses the doctrine, that the arterial tunic is muscular, contractile, and irritable, and maintains that muscular fibres can be demonstrated in arteries; that, if they could not, this would not prove the absence of contractility; and that no conclusion can be drawn from the circumstance, that fibrin has not yet been recognized in the arterial tunic. Mr Liston further contends, that the emptiness of the arteries after death, the continuance of the circulation in cases in which the contractile power of the heart is impaired by disease, the continuance of the circulation in animals void of heart, and in some classes of monsters, and the temporary excitement of the circulation in the countenance under the influence of strong emotion,—all demonstrate the contractile and irritable power of the blood-vessels. In this we conceive Mr Liston not only misapprehends the question at issue, but adduces arguments which, if minutely examined, would make little for that view which he defends. Granting that the muscular structure and irritable characters of the large vessels were proved beyond the possibility of doubt, this could never be employed to demonstrate the same properties in the inconceivably minute vessels which are the seat of the inflammatory process. The emptiness of the arteries after death we always understood to depend on a different cause entirely from that to which it is here ascribed; and we never imagined that any doubt was entertained that it was the effect of the blood not returning from the lungs to the left auricle, in consequence of the extreme difficulty during the termination of life which the blood experiences in moving through the ramifications of the pulmonary artery. We do not deny, however, that the capillaries possess some power, by virtue of which they appear to promote the circulation in them; but it must not be forgotten that it is by the loss of this power, rather than by its increase, that inflammation appears to be produced; and this, indeed, Mr Liston virtually admits afterwards to be the case.

"When a part is excited, the circulation is accelerated, and a great quantity of blood is transmitted to its vessels; but if the excitement is speedily removed, they recover themselves, though perhaps a little dilated, and no inflammation ensues. But if the exciting cause is applied for a sufficient length of time, the extreme vessels lose their contractility, they are weakened, become dilated, and the contained blood circulates slowly."

It is rather singular, nevertheless, that immediately after this passage, in which he allows that the vessels are weakened, Mr Liston states, that "unless inflammation is fairly established in a part, the capillaries become dilated, but there is *no proof that this change is in any way connected with debility*."

The account of suppuration, though not very systematic, has the advantage of being practical, and directing the attention of the surgeon to the measures to be pursued for preventing the injurious consequences of delaying to discharge the contents of abscesses. The following observations on the effect of deep-seated collections, that is to say, diffuse suppuration of the cellular tissue, may communicate some idea of the general manner of the author.

"When the abscess is situated deeply, or beneath a fascia, a free and early opening must be made. For example, when suppuration has occurred in the cellular tissue beneath the fascia lata of the thigh, it at first naturally tends towards the surface, but its progress is impeded by the tendinous aponeurosis; a painful feeling of tension is thereby occasioned, and the matter extends where there is the least resistance, making its way in all directions into the surrounding cellular tissue—separating the muscles—isolating the arterial trunks—burrowing beneath the fascia over the whole limb, and producing most serious, and often irreparable mischief, with violent constitutional disturbance. The bad effects of delay are daily witnessed in neglected cases of paronychia; most excruciating pain is produced—the system is seriously affected—the tendons slough, and the member is rendered useless.

"By the continued presence of purulent matter, absorption, ulceration, caries, and even death, of bone, is frequently produced, all which would have been prevented by its timeous evacuation. If pus collect in the neighbourhood of cavities or canals, it is of the utmost importance that it be early discharged; and the evil effects of negligent treatment are well exemplified in the following cases:—A patient had been allowed to labour, for a long time, under an extensive abscess at the lower part of the neck, beneath the origins of the sterno-mastoid muscles. The abscess at length gave way externally; but the patient was at the same time seized with profuse expectoration of pus, and during expiration the air escaped through the external openings in the neck. It was evident that the abscess communicated with the trachea, and it also appeared to have extended into the mediastinum. The patient soon perished, but there was no opportunity of examining the parts. In another case of extensive abscess at the root of the neck, an opening was proposed, but delayed. At length the abscess gave way spontaneously, and from the circumstance of portions of solid as well as fluid ingesta escaping by the external opening, it was evident that the œsophagus had ulcerated. The cure was very tedious, but ultimately complete."—Pp. 44, 45.

Subsequently when treating of erysipelas he represents this disease as the effect of infiltration of some deleterious matter into the cellular tissue. The sloughing which succeeds he regards as the consequence of this infiltration; and though the affection has been termed Diffuse Cellular Inflammation, he thinks that a more proper epithet would be Diffuse Cellular Infiltration.

On this point, we have only to observe, that we are never solicitous to dispute about names, otherwise it might be shown, without much difficulty, that the term *infiltration* conveys an erroneous impression of the nature of the disease, in far as it denotes not the morbid process itself but one of its effects.

The mode of treatment recommended for this disorder is energetic and judicious. With the use of constitutional remedies, Mr Liston recommends punctures by the lancet, followed by fomentation with chamomile flowers, and free incisions by the knife, followed by fomentation and the use of the poultice, without oil or grease. In the choice of the incisions Mr Liston prefers a few free long incisions to the practice of numerous small ones recommended by some on the ground that the former are tedious in healing. The rate at which either heal will not, we are satisfied, depend on their size, but on the stage of the disease at which they are employed; and the degree of injury inflicted by the inflammatory and sloughing process on the subcutaneous tissues. The length or brevity of incision, therefore, ought not to be regulated by this consideration, but by the state of the parts to be divided. Punctures, instead of incisions, are recommended in erysipelas of the face, unless where the scalp is swollen and puffy, when Mr Liston properly impresses the necessity of free incisions through the scalp. The use of nitrate of silver he restricts to *erythema* only.

Under the head of inflammation of the synovial membranes, Mr Liston gives a good history of the usual progress of diseases of the joints. We cannot, however, agree with him in regarding the distinction established by Mr Brodie between the cases in which the disease commences in the synovial membrane and those in which it originates in the cartilage as of no practical utility. It cannot be denied, that, if the only remedy for diseased joints is amputation, then this distinction is of no avail. Two facts, however, we conceive to be as certainly established as any in the practice of surgery admit. The first is, that inflammation of the synovial membranes is much more susceptible of resolution or termination by ankylosis than disease of the cartilages; and every one engaged in the treatment of such cases must have witnessed at least one or two recoveries. The other is, that at erosion of the cartilages the disease rarely stops till the cartilages are utterly destroyed, and it will depend on the strength of the patient whether he can survive this process until the denuded bones are ankylosed. The section is concluded with an excellent delineation of the morbid appearances produced by inflammation of the synovial membrane, which the student will peruse with advantage.

In speaking of the treatment, the author impresses the indispensible necessity of absolute quietude and immobility of the joint. He expresses himself favourably of the employment of caustics and means of counter-irritation, and recommends the compressive method of Mr Scott, though without the slightest mention of the author. This indeed is one of what may be named the weak peculiarities of this work, that not a single name of any pathologist or surgeon, famous or infamous, distinguished or obscure, is suffered to appear; and the unhappy reader is allowed to exercise his ingenuity in conjecturing who this person, or those authors may be, who have advanced this opinion, or pursued that practice, or to imagine that it is the theory and practice of the author alone.

The account of necrosis is unnecessarily encumbered with the refutation of absurd or exploded opinions. No one, for instance, at present believes in the absorption of foreign bodies, though the idea, that they are occasionally transferred by means of interstitial absorption from one part of the body to the other, generally on surface, cutaneous or mucous, is not uncommon, and is well established. Superficial necrosis Mr Liston represents as occurring most frequently in the flat bones.

The account of aneurism and its treatment is, as might be expected, the most interesting in the volume. Mr Liston restricts the term aneurism to that in which there is rupture of the inner coats, though he allows that dilatation may take place previous to this event, and that thus both causes may concur to the formation of the disease. The following remarks on the spontaneous cure deserve attention.

"In some rare cases the deposition of fibrine has gone on gradually accumulating, filled completely the aneurismal cavity, restored the course of the blood to its former channel, and thus effected a spontaneous cure, the remaining solid tumour imperceptibly diminishing by the action of the absorbents. Or, after obliteration of the aneurismal cavity, the fibrine may be deposited in so great quantity as to occupy the calibre of the vessel above and below the tumour, obstructing the progress of its blood, causing it to flow by the collateral smaller branches, and effecting a spontaneous cure, somewhat similar to that produced by the artificial application of a ligature. Coagula are not formed in the dilated vessel, to whatever size it may be enlarged, unless there is a fissure of the internal coat, for in no other way can a portion of the blood become stagnant, while the calibre of the vessel remains pervious.

" A spontaneous cure may also be accomplished from the original aneurism being compressed by one of a more recent origin, causing ultimate obliteration of the canal. Of this I recollect one remarkable instance; the patient was afflicted with an aneurism of the sub-clavian artery, which had attained a large size, and the cure for the disease in this situation being then unknown, the patient was considered as lost; but some time after the tumour began to diminish, and disappeared. The patient died; and the cause of death was found to be the giving way of an aneurismal tumour of the arteria anonyma, which was situated so closely to the aneurism of the subclavian as to have acted as a mechanical compress, causing obliteration of the vessel at that point."—Pp. 147, 148.

The subsequent observations on the treatment are judicious. In that of aneurism by anastomosis, he properly impresses the propriety of excision where practicable, that is, where the whole mass of vessels can be comprehended in the incisions. The disease, he observes, must be cut out, not cut into,—a measure which only renders it a great deal more unmanageable and hazardous than if left to itself. The ligature of the principal arterial trunks entering the tumour has been found on trial to be inefficient, in consequence of the unusual number of anastomotic communications which are almost invariably found to attend this disease; and, though the ligature of the carotid trunk in one or two instances in which the disease was seated in the head or face has arrested its progress, in others it has been found ineffectual. When the tumour cannot conveniently be removed by excision, Mr Liston recommends the ligature, either thrown round its base if pedunculated, or by means of the needle introduced beneath it when broad and sessile.

In the treatment of diseased, injured, or wounded veins, we are happy to see Mr Liston inculcates the use of pressure only, and is decidedly against the application of a ligature, which should never be done, he says, unless as a last resource. In the treatment of *varix*, he is decidedly partial to the method of Mr Mayo, by inducing the adhesive inflammation by means of the caustic potass. He allows, however, that the practice is not free from danger, either from ulceration of the venous coats and alarming hemorrhage, or from excessive spreading *phlebitis*, with the usual constitutional disturbance, and probably fatal termination. To obviate these inauspicious results, Mr Liston recommends that the caustic be always applied to the venous trunk at a healthy point.

Under the head of tumours, the account of those of bones is perhaps the most original and instructive. These Mr Liston divides into *Exostosis*, *Osteo-sarcoma*, *Spina ventosa*, and a tumour connected with the vascular system of bones. His view of exostosis is not very distinct. But, so far as we understand the author, he appears to apply this term to those bony growths which either consist of enlargement of the substance of the parent bone, or the accretion of bony matter to its surface. These bodies may either remain stationary after attaining a certain size, or they may die from deficient vitality, giving rise to necrosis; or they may, by their irritation, as foreign bodies, cause suppuration, and form a troublesome and not very manageable abscess.

By *osteo-sarcoma*, Mr Liston understands an "enlargement and alteration in the structure of a bone, accompanied with the deposition of a morbid sarcomatous substance internally." These successive changes are described with some care; but it appears to us that they are given without regard to the progressive sequence, and the general cause on which they depend. The sarcomatous matter is of a fleshy consistence and brown colour; and its deposition in the *cancelli*, or in the interstitial cavities, gives the diseased mass its peculiar anatomical, and several of its pathological characters. It is deposited in a fluid form; and its property of spontaneous coagulation is not badly expressed by the epithet of *colloid*, which the French pathologists have applied to it. Its subsequent consolidation and successive enlargement produce, first, absorption and extenuation of the sound bone; secondly, removal of much of its earthy matter, so as to render the bone fleshy and cartilaginous; and, finally, ulceration and mechanical fissure of the remaining parts; so that it is not uncommon in an osteo-sarcomatous tumour to find fragments of carious bone, bony spiculæ, ichorous abscesses, and colloid softening, all combined. The disease is always malignant; and Mr Liston is perfectly right in pronouncing it to be such.

On the subject of *spina ventosa* much misconception has prevailed; and so variously, so absurdly, and so unintelligibly do most surgeons speak of it, that we have more than once thought the name ought either to be banished from surgery, or defined in a more accurate manner. As we may not possibly understand the account of the present author any more than his predecessors, we must allow him to speak for himself.

"By this term *spina ventosa*, is understood a mere expansion of a bone from a collection of matter in its substance. The disease may be produced by external injury, exciting inflammation, and consequent suppuration, in the cancellated tissue; or the inflammatory action may be of a less acute kind, in a weakened and unhealthy constitution. The fluid accumulates, the cancelli are broken down, and the much-attenuated parietes of the bone are pressed outwards. Occasionally inflammatory action is excited on the external surface, from the pressure of the contained fluid, and minute nodules of bony matter are effused, as if nature endeavoured to strengthen those walls which are daily becoming thinner, and more incapable of supporting the weight of those parts which they encircle. The disease differs from osteo-sarcoma in the contents being uniformly fluid, generally purulent, though often mixed with more liquid and dark-coloured matter, or with a curdy substance—in the gradual extension of the bone, in no fungus protruding after a portion of the attenuated bone has given way, but matter being discharged as from a common abscess; and in the tumour not being of a malignant disposition. At first there is considerable pain in the part whilst the matter is forming, but afterwards it becomes much less acute, and in many instances there is no inconvenience, except from the bulk of the tumour. Often after having reached no very large size, it becomes stationary, neither recedes nor enlarges, and all painful sensations cease; in other cases it enlarges gradually, attains an enormous size, and produces much disturbance of the constitution; but in such instances the patient is generally weak and cachectic. The largest tumour of this species which I have seen, occurred in the lower part of the femur. In breadth, seven inches, in length, seven and one-fourth. The parietes were composed of an extremely thin lamina of bone, and in this there were numerous deficiencies supplied by delicate ligamentous matter; its cavity was divided into several compartments by thin septa, partly osseous and partly membranous. The patient was a boy of twelve years of age; amputation of the limb was earnestly advised, the friends objected; he died hectic. Generally, however, the tumour is of the size of a hen's egg, and but seldom exceeds that of an orange."—Pp. 211, 213.

We are ever willing to allow every observer to take his own method of describing a disease. But we must inform Mr Liston, that this is a statement of symptomatic effects, and that he overlooks the primary pathological cause. The "inflammation and suppuration of the cancellated tissue," of which Mr Liston speaks, is, properly speaking, inflammation of the medullary membrane; and the expansion which is represented to follow effusion of matter rarely continues long without complete rupture or ulceration of the bony walls.

The only instance of tumour connected with the arterial system of bones has been already published in this Journal, and it is unnecessary to dwell on it at greater length. We always regarded that tumour as of the same nature with the one recorded by Pearson and Scarpa, and more recently by M. Lallemand and M. Breschet, in the *Repertoire* of the latter, Tom. ii. p. 21. Mr Liston himself gives no opinion of its precise nature; but we may take this opportunity of suggesting to him to compare the anatomical characters of these cases with those of the one which fell under his observation. A few more cases of this kind accurately recorded would enable us to determine their pathological nature.

The directions for the treatment of these several forms of tumours are well worthy the attentive perusal of the practical surgeon; but our limits compel us to proceed.

Mr Liston next proceeds to give an account of the surgical characters of wounds, in which he dwells chiefly on the theory of hemorrhage, and the mode of suppressing it. His view of the circumstances, which tend to check hemorrhage from wounds of arteries, agrees with that of the generality of pathologists; but he properly remarks, that for this the surgeon must not wait, but proceed forthwith to use the means furnished by art, viz. pressure, styptics, and above all the ligature; and here again he repeats the injunction, on no account to apply the latter to veins. In dressing wounds, Mr Liston was at one time rather partial to the use of sutures. These, however, with the resinous plaster, he informs his readers, he has in a great degree abandoned, and uses instead of the resinous plaster, slips of glazed ribbon immersed in a saturated solution of isinglass in brandy, which he represents as much less irritating, and more tenacious than the common adhesive plaster. Sutures are employed to fix and retain the parts temporarily in apposition; and the ribbons are not applied till the adhesive substance has partly coagulated, and the oozing of blood has ceased. After the application of the ribbon slips, the suture-points are withdrawn, and the wound is left to the retentive powers of the former, which is represented to be adequate in this manner to the union of the largest wounds.

Mr Liston then proceeds to the consideration of bruised, poisoned, and gun-shot wounds, of each of which he gives short practical sketches. He differs from the military surgeons as to the expediency of primary amputations in severe injuries of the limbs in civil practice, in which he says a larger proportion of primary amputations are unsuccessful, and the secondary are found to be on the whole more favourable.

In treating of tetanus, he admits that the traces of inflammation or congestion of the spinal chord, though occasionally coexistent with the tetanic symptoms, are not uniformly so, and consequently, cannot be regarded as the pathological cause of the disease. The redness and inflammatory appearance of the pharynx and trachea, remarked by the author in one case, appear to be rather effects of the violent spasmodic action than causes of the symptoms. Of all remedies and modes of practice, he speaks with little confidence in this disease; and in the only case in which he tried amputation, already published in this Journal, he allows that it was unsuccessful.

In treating of ulcers of the genitals, Mr Liston includes not only syphilis, but yaws, sivvens, the Canada disease, and the poisonous effects of mercury. On the latter topic he gives some judicious observations, which, though not new, will be read with some interest.

"Of the bad effects of mercury on the constitution, much might be said. Treatises have been written on mercurial pox, a species reported to be much the most violent; and others have detailed an accumulation of evils, under the title of mercurial disease. There is no doubt that extensive, deep and sloughy ulcers of the throat are produced by mercury, and of this, I saw the following unexceptionable instance:—The fauces presented one extensive mass of ulceration, sloughing at its margins, and the uvula was almost detached. The patient was an old and emaciated woman, who neither had, nor could be supposed to have, any venereal complaints. She employed herself in coating mirrors with quicksilver, and to that she ascribed her malady. In fact, her system had been long under mercury, in consequence of her occupation. When I visited her, her daughter and husband, the latter of whom was paralytic, and almost bedridden, were affected, from the same cause, with a pustular eruption of the face, with disease of the nostrils and snivelling. Another old woman had numerous and deep ulcers of the fauces, tongue, and lips, having been kept unmercifully under mercury for nine continuous months. She had, besides, taken it from time to time for upwards of four years, though her sole complaint was slight sore throat. Pains of the joints, too, I believe, are attributable to the use of mercury. That medicine has no power to prevent the occurrence of nodes, for these often form during its action. Affections of the periosteum are easily excited in some people who have neither had pox nor mercury; but in no instance of venereal disease, have I observed serious affections of the bones, where mercury had not been given. Even the advocates for mercurialising speak of mercurial nodes. It has been asserted, that nodes do not occur when mercury has been given for liver or other complaints; but they do form under such circumstances, though not so frequently as when the medicine has been exhibited during venereal symptoms."—Pp. 309, 310.

Mr Liston is therefore averse to the practice of giving mercury liberally, but he allows its use in moderate doses; and he is most partial to the use of corrosive sublimate, the bichloride, at the rate of one-sixteenth of a grain three times daily. On the sanative powers of nitrate of silver in venereal and sloughing sores, especially of the throat, he justly bestows the highest encomiums.

We have thus taken a brief and rather hasty view of the present treatise. Its character is easily given. Without containing much new, it presents a short view of the principles and practice of the author in some of the most important points in surgical pathology and practice. The treatise will no doubt be useful to the student; but its utility would have been augmented, had the author noticed several of the authorities either for opinions or modes of treatment, which he has necessarily introduced. We are not anxious to examine the work in a censorious vein, otherwise we would suggest to Mr Liston, that he ought to distinguish more accurately the anatomico-pathological and physical characters of surgical diseases from the constitutional and other symptoms, which are mixed together in a manner something of the oddest. We shall, notwithstanding, expect with some impatience the second part of his work, which the operating talents of the author cannot fail to invest with peculiar interest.

ART. VIII.—*Experiments relative to the Carbonic Acid of Expired Air in Health and in Disease.* By J. APJOHN, M.D. M.R.I.A. Professor of Chemistry to the Royal College of Surgeons, &c. &c. (From the Dublin Hospital Reports, Vol. v.)

IN this paper, Dr Apjohn relates several experiments, from which it results that the statements of Messrs Allen and Pepys regarding the quantity of carbonic acid produced in respiration require to be modified.

The conclusions established by the researches of Messrs Allen and Pepys may be referred to the following general principles. 1. Air undergoes no change of volume in ordinary respiration; 2. Part of its oxygen disappears, and is replaced by an equal volume of carbonic acid gas; 3. The carbonic acid of air once respired amounts to from 8 to 8.5 per cent.; and, 4. When the same quantity of air is respired so frequently that it is no longer fit for the purposes of the animal economy, it contains 10 per cent. only of carbonic acid gas.

From the numerous and varied experiments of Edwards, however, it results, that the first two conclusions required to be considerably modified. It was demonstrated, in the *first* place, that when animals are inclosed in given portions of air, the respired air is sensibly diminished in volume; that this diminution takes place at the first; and that it varies in different animals. It was shown, in the *second* place, by the experiments of Edwards, that this diminution of volume is owing chiefly to the absorption of oxygen gas, but partly, at least at certain periods of the year, to that of nitrogen. *Thirdly*, it was established that the carbonic acid gas, though sometimes equivalent to the oxygen which disappears, is occasionally less.

The most remarkable circumstance which strikes an observer in perusing the statements of Allen and Pepys is, that the quantity of carbonic acid gas in air respired once certainly seems to be estimated extraordinarily high, more especially when we are informed that in the 15th experiment, in which the operator breathed 300 cubic inches, until he felt an oppressive sense of suffocation in the chest, lost vision and consciousness, and had all the symptoms of incipient *asphyxia*, there were only 10 per cent. of carbonic acid gas, and 4 of oxygen still remained. As the operator made 35 respirations in order to produce this quantity of carbonic acid gas, it appears singular that only one deep respiration should produce so much as 8 or 8.5 per cent. These circumstances, we acknowledge, always appeared irreconcileable; and the natural inference, if the results of the second experiment were accurate, is, that the quantity given in the first was much overrated. It must not, however, be concealed, that although in the remarks on the 13th experiment, they inform us, that in some experiments made with a view to ascertain the difference between the quantity of carbonic acid gas in the first and last portions of a single inspiration, a small quantity of the first portion was found to contain only 3.5 per cent. of carbonic acid, and in other experiments, the first portions contained a quantity varying from 3 to 5 per cent. Messrs Allen and Pepys represent the general average to be about 8 per cent.

Dr Prout appears first to have called in question the accuracy of these statements. From some experiments performed with much accuracy by this chemist, it appears that the greatest quantity of carbonic acid gas produced in ordinary respiration is 4.1, and the smallest 3.3, giving a medium of 3.45. He further infers, that the amount varies within the twenty-four hours, being at its maximum between 11 A. M. and 1 P. M., and at its minimum at 8 P. M. Dr Prout, like Dr Fyfe, also found that the quantity varied according as fermented liquors had been used during the observations, when it was always much diminished.

These objections appear to have occurred very forcibly to Dr Apjohn, since he states, that, from experiments made some years ago, he was led to regard the statement respecting the amount of carbonic acid gas in the air of one expiration as much exaggerated. With the view of investigating the point, he instituted, with the assistance of Mr Grimshaw, a series of experiments, which appear to have been conducted with every attention to secure accurate results. The greatest quantity of carbonic acid found in respired air in these experiments is 4.5, and the smallest 2.1, while the average of the whole for the morning of 7 days in the month of August is 3.5, that for noon for 11 days is 3.7, and that for the evening for 10 days is 3.6.

From these numbers, it appears that air once respired, instead of containing 8.5 per cent. of carbonic acid gas, contains only 3.6 at an average, and that the quantity was seldom below 3 per cent., or above 4. There are, however, considerable irregularities in the quantity of carbonic acid produced. "Thus, in the morning, Mr Grimshaw produced 4.7 per cent. of carbonic acid; and upon repeating the experiment immediately after, produced but 4.4, without being conscious of any thing which could account for the difference. On the next day, at 11½ A. M., I found that the air which I expired myself, contained but 2.9 per cent. of carbonic acid, but upon repeating the experiment in ten minutes after, it was raised to 4.2. This irregularity, however, which was so striking, I could clearly trace to a temporary mental depression. With respect to vinous drinks, I could not find that they exercised in my own case any perceptible influence."—Pp. 532.

Dr Apjohn next proceeded to inquire into the accuracy of another conclusion of Messrs Allen and Pepys, that, namely, in which it is stated that air respired as often as possible still contains 10 per cent. of carbonic acid gas. After subjecting 220 cubic inches of air to repeated respiration, continued on an average about two minutes each time, till uneasiness was felt, Dr Apjohn obtained the following results:

August.	C^{c}. acid of air once respired.	C^{c}. acid of air respired as often as possible.
3	3.7	6.4
4	4.1	7.1
7	4.3	7.1
12	4.1	6.0

In these experiments, it appears that air repeatedly respired, until asphyxia approaches, contains scarcely 8 per cent. of carbonic acid gas, or only double the quantity of the first respiration. It is merely possible that this discordance may depend on the different respiratory powers of different experimentors; for it cannot be doubted that some individuals have greater powers of enduring the respiration of vitiated air than others.

Nysten, it may be necessary to remind our readers, had made a statement, that, in several persons who respired the same quantity of air for several minutes, he observed that the respired air contained less carbonic acid than air breathed for a much shorter period, for example, for 30 seconds; and from this he inferred, that carbonic acid was absorbed.* Dr Apjohn, however, shows that the premises from which this conclusion is derived are fallacious. In the first experiment performed by him and Mr Grimshaw, the air respired 30 seconds gave 6.6, and that breathed as often as possible, viz. 2 minutes, gave 7.1 per cent. of carbonic acid gas. In a second experiment, air respired 30 seconds gave 5.6; and air respired 1 minute and 15 seconds gave 6.4 per cent. In a third experiment, air respired 30 seconds gave 5.6; and air breathed 1 minute and 40 seconds gave 7.6 per cent. of carbonic acid. In short, by varying and repeating the experiment, they were perfectly satisfied that the quantity of carbonic acid increases with the time during which a given quantity of air is breathed, though not so rapidly during the latter respirations as the first ones; and that, when the non-respirable or asphyxiating limit is attained, the carbonic acid amounts only to 8 parts in the 100.

In the course of these experiments, Dr Apjohn was enabled to determine another point of much moment,—the influence of respiration in unfitting air for combustion. It is sometimes stated, that any mixture of oxygen, azote, and carbonic acid, which sustains combustion, is respirable, and the converse. The first statement only is well-founded. Dr Apjohn finds that a mixture of these gases, in which a taper will not burn, may be respired, and he therefore infers that the common test for choke-damp may be relied on, or that the atmosphere in which a candle burns may be entered with impunity. It is interesting to remark, also, that by gradually introducing known quantities of carbonic acid into atmospheric air, these gentlemen have found that the smallest proportion of carbonic acid in which a taper could no longer burn, was 15 per cent. Since it appears that the same result is produced when an atmosphere is respired till the carbonic acid is raised to 5.2, it follows that these two conditions of an atmosphere are equivalent, or, in plain terms, that on the power of maintaining combustion, the same effect is produced by the removal of 5 per cent of oxygen, and the introduction of 15 per cent of carbonic acid gas.

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* Reserches de Physiologie et de Chimie Pathologique, &c. Pp. 222, 223.

Another point of the inquiry Dr Apjohn and his colleague now undertook to investigate, namely, to determine the influence of external temperature in augmenting or diminishing the carbonic acid of respired air. With the view of determining this point, both gentlemen repaired to an extensive calico-printing establishment, and having previously ascertained the proportion of carbonic acid produced by ordinary respiration with the thermometer at 60° in the shade, entered an apartment with a temperature at between 146 and 152°, where they remained for 37 minutes, and each respired into a separate elastic gum bag. The analysis of the contents gave the following results:

	Carbonic acid at 60°	Carbonic acid at 149°
A.	4.3	2.7
G.	4.4	3.0

Though these numbers indicate that a smaller quantity of carbonic acid is formed in a high than a low temperature, we do not perceive that they determine the point as to the influence of temperature in any other mode than as presenting a more rarified medium for respiration. The effect of heat is to expand the air. A smaller quantity is therefore presented to the surface of the organs of respiration, a smaller proportion of oxygen is respired, and a smaller quantity of carbonic acid gas must be formed. In all this we perceive nothing very difficult to understand. The acceleration of the respiration, and the frequency of pulse which took place in both experimenters, may have also contributed to the same effect.

A *fourth* department of the inquiry next attracted attention, viz. to determine the changes in the quantity of carbonic acid expired in different morbid states of the system. Nysten, we must remind our physiological friends, was the first who attempted to determine this point in an accurate manner. Of 7 persons labouring under continued fevers, one produced 6 per cent. two 6.5, two 4.5, one 5.5, and one 2.5. One case of ague produced 7 per cent. and another 8. Of 12 cases of pulmonary consumption with hectic, the smallest is 1 per cent. the greatest 8 and 8.5, and the medium is 3, 4, and 5, there being 4 cases at 5 per cent. One case of hydrothorax, and another of ascites, produced only 2.5. All these patients respired 1500 cubic centimetres, equal to 590 cubic inches, or about 16 imperial pints of air for 30 seconds. The general conclusion, in short, which M. Nysten thought himself justified in forming from these experiments, is, that in all cases of disease in which the respiratory motions are impeded or embarrassed, a smaller quantity of carbonic acid gas is formed than in the state of health, when they are free.*

It is a circumstance, nevertheless, not altogether free from suspicion in the numbers given by M. Nysten that they are in general so high and so variable. If the rectifications of the present author are to be adopted, the quantity of carbonic acid produced in 30 seconds in a given quantity of air respired would amount in many instances to as much as the carbonic acid gas produced in a quantity of air respired by persons in health till it is no longer fit for respiration. Accordingly, we find these numbers are, if estimated by the analogous experiments of Dr Apjohn and Mr Grimshaw, prodigiously exaggerated.

From the table of results it appears, that the lowest amount of carbonic acid is 1.3, and the highest is 4.4, while the average amount may be represented to vary from 2.5 to 3. The two lowest averages, both occurring in diseases of the heart, are only 1.6. From these results it may be inferred, that the faculty which the lungs possess of changing the constitution of the air respired, though considerably, is not so much impaired by direct disease of these organs as might *a priori* be expected. In the phthisical cases, though reduced, it is not by any means so low as in diseases of the heart. In dropsy and pregnancy it appears to suffer considerable diminution. But in continued fever and all acute affections it appears not to be diminished, and occasionally to be augmented. Typhus fever is said to form an exception; but as one case only was examined, we doubt whether it was sufficient to justify the inference.

In conclusion, these are valuable experiments, and if their results shall be confirmed by future observers, they must be allowed to add much to the precision and accuracy of our knowledge on the changes induced on the air by respiration.

ART. IX.—*A Vade Mecum of Morbid Anatomy, Medical and Chirurgical, with Pathological Observations and Symptoms.* Illustrated by upwards of 250 Drawings. London. Burgess and Hill. 1830. 8vo. Pp. 51.

OF this work we have merely to say, that it appears as a substitute for the large and expensive engravings in morbid anatomy; and as it embraces the whole range of the human body, it will be a very useful companion to the student in the hospital and pathological museum.

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* Recherches de Physiologie, &c. p. 212.

Its arrangement is simply that of the old anatomical works; —the head, neck, and throat, chest, including the lungs and heart, belly, including the alimentary canal and its appendages, and the pelvic organs, containing those of generation. To illustrate the morbid changes incident to the organs of these different regions, the author employs 48 octavo plates. They are done in the lithographic style, for which there is such a fashionable predilection; and as this enables the author to give a collection so numerous at so moderate a rate, it would be unkind, indeed, to speak otherwise than favourably of a work, the great object of which is to enable the student to form some general ideas of this important department of medical knowledge. It would be absurd to expect great beauty or accuracy of engraving under such circumstances; and that, therefore, is out of the question. It is merely just, however, to say, that every thing relating to changes in form, to changes in size, and the more distinct changes of structure are wonderfully well represented. The drawings are stated to be all original, made from preparations in the author's possession. If so, it was idle to conceal his name, since the knowledge of this may operate with many in giving character to the book. In the present day, excess of modesty is a vulgar, though not a common error; and the author may be assured, unless his name is excessively high indeed, that it is not likely to suffer by being prefixed to his work.

The letter-press is descriptive of the morbid changes, and contains short sketches of the symptoms with which they are attended. On the whole, this work must be regarded as a valuable accession to the library of the student.

Art. X.—*A Demonstration of the Nerves of the Human Body, consisting of four parts. Part I. The Cervical and Thoracic portions of the Sympathetic and Nerves of the Thoracic Viscera.* By Joseph Swan. London, Longman, &c. 1830. Elephant Folio.

In 1825 and 1828, Mr Swan obtained the anatomical prizes of the College of Surgeons for his dissection of the nerves of the spinal chord, and of the cerebral nerves, from their origins to their terminations and junctions with the visceral nerves, both authenticated by preparations. The object of the author in the present engravings is to record the distribution and connections exhibited by these preparations of the most important parts of the nervous system. In this undertaking, he has been highly successful, as might be expected, as to the anatomical part; and though, as pictorial and sculptorial objects, they will not bear comparison with the beautiful engravings of Scarpa, they are to be regarded as highly creditable to the diligence and zeal of Mr Swan. In this country, indeed, in which anatomical engraving has always been much inferior to that of the continental artists, the present delineations are entitled to a very high rank.

The present part contains four plates shaded, with outline duplicates for the purpose of explanation. The shaded plates, though rather stiff in the figure and attitude, are remarkable for anatomical distinctness. The first plate gives a good view of the spheno-palatine ganglion, and the Vidian nerve above, the laryngeal and pharyngeal *plexus* in the neck, and the different cardiac nerves and posterior pulmonary plexus in the chest, all on the right side. In the second the same objects are very well exhibited on the left side. The third shows the intercostal ganglions and nerves, and a more complete view of the pulmonary branches accompanying the bronchial tubes on the right side; while the fourth exhibits the same objects on the left side.

The only fault in these figures is, that some of the nervous trunks are rather larger than they are ever seen in any subject, however recent. Thus the phrenic nerve in the first engraving is greatly larger and thicker than we ever saw it, though examined at least in more than a score of subjects. In like manner we regard the *nervus vagus*, both in this and in the third figure, as much larger than it is in the largest dropsical subjects. In plate 4th, again, it is more like a large chord, or like some of the voluntary nerves, than the pneumogastric, and the recurrent bears a distant resemblance to a bell-rope. This exaggerated size looked so much like intention for the sake of distinctness, that we read the preface and descriptions a second time to discover if there was any purpose of this description. We find, however, none, and we must conclude, that it is the error of the draughtsman or engraver. This Mr Swan should certainly explain; for, if a student dissects these nerves with the engravings of Mr Swan at his hand, he will scarcely understand how the nerves which he sees in the subject are the same objects as those which are represented in the engravings.

The work is nevertheless one of great value to the neurological department of anatomy, and we trust the author will proceed in his undertaking.

PART III.

MEDICAL INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

Two newly discovered Muscles for compressing the dorsal vein of the Penis in Man and Animals. By Mr SHEKLETON and Mr HOUSTON. (*Dublin Hospital Reports, Vol. v. p.* 459.)—By the researches of a number of eminent anatomists, it is well known that the *corpus cavernosum* and *corpus spongiosum* of the *penis* have been shown to consist of minute arteries and veins, which are large, numerous, and communicate freely with each other; and that by this arrangement a number of communicating vessels is formed, into which the blood is poured abundantly on occasion of various *stimuli*, so as to cause erection,—a phenomenon from which the vessels in which it takes place have been distinguished by the name of *erectile.* * Though there is little doubt that the veins thus arranged are the chief seat of this action, it is still undetermined by what agents or mechanism the occasional injection is effected. We know, in short, that the blood is poured from the arteries into the minute communicating veins during erection; that it is detained in the latter order of vessels for some time; and that then it is gradually removed from the minute veins into their larger branches and trunks, while the part shrinks to its volume previous to erection. We are, however, ignorant of the means by which this retardation of the blood in the communicating veins is effected, and of those employed to effect the removal of the blood after erection.

Mr Houston of Dublin, by whom this kind of structure has been recently examined, and whose researches in proving that it is the cause of the rapid protrusion of the tongue of the chamaeleon, we on a former occasion brought before our readers, has partly modified the old, and partly confirmed the new, explanation of erection. Mr Houston, though obliged to admit that the mere peculiar vascular arrangement is all that the anatomist can demonstrate in most organs susceptible of occasional erection, thinks, nevertheless, that this phenomenon depends on two circumstances,—the one the impulse of blood by nervous energy through the arteries, the other a mechanical process, by which the blood is detained during erection in the veins of the organ.

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* Elements of General Anatomy. By David Craigie, M. D. Chapter vii. p. 183—189.

His attention was directed to this peculiarity by the fact communicated by Mr Shekleton of the existence of two muscles in the dog connected with the *venæ dorsales*, of which the position is well suited for compressing these vessels, and preventing the escape of blood from them. These muscles, which are situate between the arch of the pubis and the *penis*, had been already noticed by Cuvier in the bear, dog, raccoon, and tailed monkeys, in the following terms:—" Les *ours*, le *raton*, et le *chien*, &c. ont un petit muscle dont les fibres charnues partent des branches du corps caverneux et se réunissent à un tendon moyen qui se fixe à la verge au dessous du pubis. Dans la *guenon callitriche*, où nous l'avons egalement trouvé, il n'avoit pas de tendon moyen, et devoit servir à comprimer la *veine dorsale*." Anatomie Comparée, Tome v. p. 102.

These observations have been more fully verified by Mr Houston, who after recognizing them in the dog, wolf, jackal, bear, badger, cat, raccoon, coati-mondi, marmot, aguti, horse, seal, &c., at length found them, on the 15th July 1830, in the human body. In the dog of good size, each muscle is about three-fourths of an inch long, and at its origin one-third of an inch broad. It rises from the posterior part of the pubal *ramus*, about an inch below the symphysis, and, tapering, ends in a tendon, which joins that of the opposite side, and in which is a transverse slit for the dorsal vein. In man, these muscles are less distinct than in most of the *mammalia*. Rising from the pubal *rami* above the origin of the *erectores penis* (*ischio-cavernosi*) and *crura*, and ascending forwards, they are inserted above the dorsal vein by mutual union on the mesial line. They consist of a thin layer of muscular and tendinous fibres, about an inch long, and three-fourths of an inch broad, and may perhaps be regarded as portions of the *ischio-cavernosi*, which, instead of being inserted into the sides and lower surfaces, ascend over the upper surface of the cavernous bodies to compress, by their contraction, the dorsal vein. Between these muscles and the penis are the veins, arteries, and nerves of this organ. Their anterior fibres are distinguished from those of the *ischio-cavernosus* muscles by the fibrous attachment of the *crura* to the pubal bone; their posterior margins are separated from the anterior of the *levatores ani* or Wilson's muscles by the pudic artery, which divides them in its course to the *dorsum* of the penis. They are named by Mr Houston the *compressores venæ dorsalis penis*.

The use of these muscles is manifestly to compress the dorsal vein of the penis, and consequently to impede the return of the blood from the erectile vessels. It does not appear that they are under the influence of the will, and Mr Houston appears neither to have investigated nor determined this point. That they are less developed in man than the lower animals he admits; and, on the whole, they appear to be mere rudimentary elements of a structure which, for reasons not yet explained, is most perfect in several of the lower animals. One point is obvious, that not even in the human subject can they be allowed the exclusive power of compressing the cavernous veins, since this is effected partly by the *bulbo-cavernosus* and partly by the *compressor prostatæ.*

Mr Houston more than once tied the dorsal viens in a dog with the effect of producing morbid turgescence of the penis; but we do not mean to enter into the consideration of the uses of the muscles.

It is an analogical confirmation of the anatomical fact here stated, that the tongue of the chamaeleon, in which Mr Houston already demonstrated an erectile arrangement of vessels, possesses in the internal cerato-mamillary muscles organs similar to the *compressores venæ dorsales.* These muscles rise from the anterior part of the side of the lower jaw, and, passing backwards on the upper surface of the style and muscles end in a tendon, which, uniting with that of the opposite side about two lines before the articulation of the *cornu* with the body of the hyoid bone, becomes broad and is inserted into the roots of the posterior *cornua.* A further confirmation of the analogy of structure is found in the fact, that this tendon is perforated by an opening, through which these veins pass in their course from the erectile vessels of the tongue to the recipient trunks, and that stretching these muscles interrupts a current of mercury flowing along their canal.

Case of Diseased Larynx, in which the Arteria Innominata crossed the Trachea in the situation in which this tube is opened in Tracheotomy. (Mr Macilwain, p. 335.)—A woman, between fifty and sixty years of age, laboured under symptoms of diseased larynx in a very severe degree. Her breathing was excessively laborious, and the paroxysms of threatening suffocation were very frequent, both by day and night. The thyroid gland was enlarged. There was also a tumour of a very firm texture apparently beneath the sterno-mastoid muscle of the right side. As internal treatment was found ineffectual, Mr Macilwain proposed to perform tracheotomy, and he met Mr Stanley and Mr Kingdon at the patient's residence with that intention, when, in feeling the neck in the course of the intended incision, he discovered a large artery ramifying in this situation. The vessel was so situate that scarcely an inch remained (the external incision included), for the performance of the operation. In consultation, therefore, the gentlemen before mentioned agreed that it would be better to abandon it altogether. Mr Macilwain concurred in this opinion, 1st, because in so small a space, bounded inferiorly by a large artery, the operation would have been exceedingly difficult of execution; 2dly, because, supposing the operation safely completed, this artery would have been left at the margin of a suppurating wound; and, lastly, because the only measure by which these objections could have been obviated, (viz., the ligature of the vessel), presented in so exhausted a patient no rational hope of success. After death, which took place in one of the paroxysms of dyspnœa, her lungs were found sound. In the larynx there was a growth of a very firm, almost cartilaginous, texture, which contracted the glottis to such a degree, that it appeared astonishing that she could have lived so long with such an impediment to respiration as this tumour afforded. On removing the surface of the morbid growth, there was found a small suppurating cavity within it, as was the case with another tumour which lay exterior to the larynx, in connexion with the left lobe of the thyroid gland. The artery felt during life proved to be the *innominata*.

Experiments by M. Collard de Martigny on Pulmonary Absorption and Exhalation. (Journal Complémentaire, xxxvi. 225, and xxxvii. 168.)—M. Collard de Martigny has lately undertaken a set of experiments for the purpose of determining some of the disputed points in the chemical physiology of respiration; and the following are his principal results.

1. According to the doctrine of Lagrange, which is a modification of the original theory of Lavoisier, oxygen gas is absorbed *in substance* by the venous blood in passing through the lungs, and does not unite with oxygen to form carbonic acid till it has proceeded with the arterial blood to the capillaries. This opinion rests merely on some imperfect experiments by Girtanner, who thought he discovered oxygen in arterial blood. It is singular that these experiments should not have been repeated till now, as they obviously, if correct, lead to a very precise and important conclusion. On trying them, M. Collard de Martigny procured results decidedly negative. Having filled a tube 36 inches long with mercury, and reversed it so as to produce a barometric vacuum, he admitted about an inch of fresh arterial blood from the crural artery of a living dog, and left the apparatus at rest for an hour and a half. At this time, the mercury having descended considerably, the gas which had been developed was transferred into a graduated tube, and acted on by caustic potass. The whole of it was entirely absorbed, and consequently consisted of carbonic acid only.

2. The more prevalent doctrine at present is, that the oxygen which is absorbed by the blood in its passage through the lungs *unites with* the blood; that carbonic acid is formed in the capillaries by the various processes of nutrition and secretion; and that the carbonic acid thus formed is given off in the lungs by a process of exhalation and secretion, independently of the presence of oxygen. This doctrine rests mainly on an experiment first performed by Girtanner, and afterwards more carefully by Edwards,—where a frog, being made to breathe hydrogen alone, gave off nevertheless more than its own bulk of carbonic acid in the course of a few hours. This result, if the mode of experimenting is free from fallacy, is decisive of the question. It proves, that the carbonic acid given off during respiration, is not formed in the lungs by the union of oxygen with the carbon of the venous blood in its passage through the pulmonary circulation, but arrives with the venous blood in the lungs ready-formed, and is,

in short, the product of the various functions of the capillaries. But M. Collard de Martigny considers, that even the method of experimenting pursued by Dr Edwards is liable to fallacy. On the one hand, hydrogen and carbonic acid are so different in density that they mingle slowly, and in consequence an analysis of a portion of the mixture does not represent the composition of the whole mass. And on the other hand, according to a law in physics, the tissues of the body, being impregnated with carbonic acid, must give out that gas when immersed in an atmosphere of any other gas. To obviate the former fallacy he substituted azote for hydrogen; and to do away as much as possible with the latter, he withdrew and analyzed the air in which the animal was confined once every hour or every two hours, and replaced it by pure azote,—so that in this way he could ascertain whether carbonic acid was given out only at first, and therefore arose merely from the displacement of the gas diffused through the textures, or continued throughout the whole duration of the experiment, and consequently arose from the secretion of the gas by the lungs. In every experiment he found carbonic acid given out in considerable quantity, as Edward's also invariably remarked. In an interval varying from seven and a-half to nine hours he procured in seven different experiments from $2\frac{1}{2}$ to $2\frac{3}{4}$ centilitres, or between $1\frac{2}{3}$ and $1\frac{3}{4}$ cubic inch of carbonic acid. In every instance he found nearly twice as much gas formed during the first period as during any subsequent period; which he attributes to the displacement of carbonic acid in the textures by the azote. But after the first period the quantity formed in every equal period of time was nearly the same till the animal began to become torpid and the respiration to languish, when the formation of carbonic acid rapidly decreased. Hence he concludes that after the first period, the carbonic acid is derived solely from the blood in the lungs.

The exhalation of carbonic acid from the lungs, then, is independent of oxygen being supplied to it. The presumption must consequently be, that it is secreted or excreted by the blood in the lungs. But in order to establish this doctrine satisfactorily, it is necessary to show that the blood, in passing through the lungs, really loses carbonic acid,—a point which no one before M. Collard de Martigny has endeavoured to ascertain. He has proved it, however, as he conceives, by a comparative examination of the arterial and venous blood of the same animal. When each was collected directly from the blood-vessel in a barometric vacuum, as formerly described, he found that venous blood almost always gave out twice, and on one occasion, thrice as much carbonic acid as the arterial blood. But when respiration was suspended by exhausting the lungs, and tying the trachea, the arterial blood was found to contain as much carbonic acid as the blood in the veins.

3. Physiologists have differed with one another as to the question, whether the carbonic acid gas given out in the lungs is equivalent to the oxigen absorbed by them; but, on the whole, the prevailing opinion is, that more oxygen is absorbed than

is accounted for by the carbonic acid exhaled. M. Collard de Martigny arrives at the same conclusion by a series of experiments apparently more free from fallacy than any previously made. The chief difficulty is to avoid the fallacy arising from the probability of the air in the lungs of the animal at the beginning of the experiment not corresponding in quantity with what remains at the end. The author got rid, as he conceives, of this difficulty by not confining the animal in the jar of air to be breathed, but by fixing a tube in its trachea, exhausting the air in its lungs, then establishing a communication by means of the tube between the lungs and the jar, and, at the end of the experiment, expelling the residual air of the lungs into the jar by strong pressure of the chest. He has related the particulars of nine experiments of this kind, of which, eight were performed with the rabbit, and one with the Guinea-pig. In four of them, the quantity of air breathed was four *litres*, or 244 English cubic inches; in four others, it was three *litres* and a half, or 213 cubic inches; in one it was 183 cubic inches; and the duration of the experiment varied from nine to fifteen minutes. In the largest quantity of air, the quantity of oxygen was 51.2 cubic inches. Of this there remained unaccounted for by the residue of oxygen, together with the carbonic acid evolved, 6.5, 16.3, 16.9, and 16.9 cubic inches in four different experiments. In the four experiments with 213 cubic inches of air, the total oxygen being 44.8 cubic inches, there remained unaccounted for, in like manner, 2.3, 4.7, 7.1, 10.4, and 18.4 cubic inches. In the experiment with 183 cubic inches, where the oxygen was 38.4, the quantity unaccounted for was 4.7 cubic inches.* In all these experiments, then, it is clear, that a large, though variable quantity of oxygen gas disappears,—a larger quantity is absorbed than is given off in the form of carbonic acid. But we must observe that M. Collard de Martigny commits a serious oversight in supposing that his method of experimenting is free of fallacy, or represents natural respiration. On the contrary, after the first inspiration, the animal breathes an atmosphere considerably impregnated with carbonic acid gas; consequently the blood absorbs the gas, which it is very well known to do when a moderate proportion of carbonic acid in the air is presented to it; and in this

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* The particulars are contained in the following tables, which are carefully calculated from the original tables of the author.

Oxygen in Air before Respiration.	Oxygen remaining.	Carbonic Acid.	Oxygen disappeared.
1. 51.2	22	12.3	16.9
2.	27.1	17.6	6.5
3.	22.6	11.8	16.9
4.	20.3	14.6	16.3
5. 44.8	17.4	17.0	10.4
6.	14.4	12.0	18.4
7.	19.7	22.7	2.3
8.	18.7	19.6	7.1
9. 38.4	15.3	18.4	4.7

way the apparent disappearance of oxygen may be sufficiently accounted for.

4. Another point of dispute among physiologists who have occupied themselves with this subject is whether any azote is given off or absorbed. On the whole the greater number of authorities unite in finding that a small quantity is given off. But of late much confidence has been reposed in the experiments of Dr Edwards, who found that azote is sometimes absorbed and sometimes given off, according to the season of the year. M. Collard de Martigny is at variance, however, with Dr Edwards on this point, having invariably found in many trials at different seasons that a small quantity of azote is given off. In the experiments formerly mentioned to determine the proportion of oxygen which disappears in respiration, he found in four trials with 244 cubic inches of air that azote was exhaled to the amount of 1.9, 1.8, 1.6, and 0.1 cubic inch; in four trials with 213 cubic inches, the quantity exhaled was 4.1, 3.6, 1.8, and 1.3 cubic inches; and in one trial with 183 cubic inches, the quantity was 4 cubic inches.

5. Lastly, M. Collard de Martigny considers the question, whether any water is formed in the lungs by the union of the oxygen of the air with the hydrogen of the blood. The idea that water is so formed has been, we believe, universally abandoned in Britain for some time, and we were not aware that this branch of the Lavoisierian doctrine of respiration still met with its favourers in France. It may be at the same time true, as our author states, that it is a notion more easily rejected than disproved. The objection first urged against it, that hydrogen never unites with oxygen at so low a temperature as 100°, was met with the rejoinder, that such union readily occurs when the hydrogen is in a *nascent* state. But M. Collard de Martigny objects, that according to his own experiments, and those of M. Chevallier, hydrogen is never in a nascent state brought in contact with azote without ammonia being formed, which he has never found in the halitus of the respiration. Another objection is, that whether common air or azote be respired, the quantity of halitus formed is pretty nearly the same.

In a paper which will appear presently, the author of the present essay undertakes to prove that *animal heat is altogether independent of respiration.*

New Observations on the Blood by M. Lecanu. (Journal de Chemie Medicale, No. 18, et 20th Sept. and October 1830.*)*—The colouring matter of the blood has been for some time past distinguished among the French chemists by the names of *zoohematine*, *hemachroine*, and *hematosine*. In a memoir communicated to the Royal Academy of Sciences on the 16th of August, M. Lecanu, who has instituted an elaborate series of experiments on the nature of this animal substance, announces the opinion, that *zoohematine* or *hematosine* is a compound of albumen, and a peculiar matter to which he proposes to apply the name of *globuline*. After considering the respective processes of Vauquelin, Brande, Berzelius, and Engelhart, to obtain the colouring matter in a separate form, he gives the preference to the method of Berzelius, with some modifications.

After dividing into very minute parts a quantity of blood-clot of an ox, he dilutes it repeatedly in distilled water, taking care each time to squeeze the residual mass forcibly in a cloth. The liquor, thus saturated with much colouring matter, is filtered and allowed to evaporate by the heat of the sun on large plates. The result of this evaporation is the colouring matter as pure as it can be obtained by the ordinary methods. Hematosine or zoohematine thus obtained is solid, black, and brilliant as jet when examined in the concrete form,—dull, and of a brick-red colour, when reduced to powder,—brilliant, translucent, and reddish, when prepared on thin plates. Readily soluble in cold water, it produces a beautiful red solution, with a very mawkish smell and taste, which remains unchanged for months. Exposed to a heat above 70° C. or 158° Fahrenheit, it is decolorized, and the colouring matter is precipitated in the form of brown flocculi, completely insoluble in water. Albumen, it may be remembered, only becomes precipitated at 166° F. To demonstrate the difference of this substance from albumen, M. Lecanu has performed a number of comparative experiments, of which the following is the result.

Re-agents.	12 grains of hematosine in 2 ounces of distilled water.	12 grains of albumen in 2 ounces of distilled water.
Reddened paper.	No change.	A blue colour.
Alcohol.	Flocculent red precipitate.	Flocculent white precipitate.
Chlorine liquor.	White precipitate.	White precipitate.
Infusion of galls.	Brown-red precipitate.	Whitish precipitate.
Ammonia.	Bright red colour.	No change.
Acetic acid.	Brown colour, without a precipitate.	No change.
Sulphuric and hydrochloric acids.	Brown precipitate soluble in excess of acid.	White precipitate soluble in excess of acid.
Nitric acid.	Flocculent brown precipitate.	Flocculent brown precipitate.
Bichloride of mercury.	Flocculent red precipitate.	Flocculent white precipitate.
Acetate and subacetate of lead.	No change.	White precipitate.

Of the last precipitate, he observes that it is not owing to the formation of sulphate of lead, for it is almost entirely soluble in nitric acid, and the solution in aqua potassæ, saturated by acetic acid, furnishes white flocks, which are recognized to be albumen. Sulphuric and acetic ether produce as little change as alcohol on zoohematine, and separate only a minute portion of fat. The volatile oils, especially that of turpentine, render it yellowish without dissolving it.

Hydrochloric acid produces a brown, almost black solution, not rendered turbid by heat, and the colouring matter of which is precipitated by ammonia. This solution M. Lecanu regards as a hydrochlorate. 100 Parts of hematosine contain 97.742 parts of animal matter, 1.724 of alkaline and earthy subcarbonates, hydrochlorates, and phosphates, and 0.534 of peroxide of iron.

According to this author, globuline exists in the blood of the ox and sheep, combined with albumen, while in human blood it is in such a state of freedom as to be obtained by the process already indicated.

To obtain it from that of the ox, M. Lecanu directs to be added to the blood of an overdriven bullock, previously diluted with four or five times its weight of water, a slight excess of subacetate of lead, to filter, and add to the filtered liquor, as much sulphate of soda as precipitates all the excess of lead. After allowing the mixture to stand for some hours till the sulphate of lead subsides, it is again filtered, and the liquor which escapes, of a brilliant red, retains its solution at the colouring matter. By adding a sufficient quantity of hydrochloric acid, the liquor is decolorized, and there are precipitated brown flocculi, which are collected on a cloth, and after being dried in a sand bath, are exposed to the repeated action of alcohol. The addition of a few drops of ammonia to the alcoholic liquor causes the brown colour to pass into a rose red, and throws down abundantly red *flocculi*, which are treated several times with boiling water to remove the ammonia. The residual substance, when dried, is *globuline* in a state of purity.

Globuline is, then, an inodorous, insipid substance, blood-red in the form of hydrate, red-brown when dry, insoluble in water and alcohol, very soluble in water of potass and ammonia, at 24° C. = 76° F., and forming with them red solutions. These alkaline solutions are incoagulable by heat; but infusion of galls and acetic and hydrochloric acids cause a brown precipitate; and chlorine liquor throws down a clear brown precipitate, which is eventually decolorized. Hydrocyanate of potass demonstrates the presence of iron.

Globuline is distinguished from albumen by its colour, by the iron which it contains, by its great solubility in acids and alkalies, and especially by the property of forming with hydrochloric acid a compound insoluble in alcohol.

These experiments would require to be repeated. The chief novelty consists in obtaining the colouring matter in what M. Lecanu regards as a purer form, and distinguishing it by a new and specific appellation.

PATHOLOGY, MEDICAL AND SURGICAL.

On the Pathology of the Blood in Yellow Fever. (Observations on the Blood, by William Stevens, M. D.)—The pamphlet of Dr Stevens is the publication of a paper lately read before the London College of Physicians on the state of the blood in yellow fever and the pathology of the blood generally. The facts he has mentioned relative to the qualities of the blood in bad cases of that disease are very interesting and well deserving of notice. In the early stage of the disease he says he has found the structure of the red globules deranged, as appears from the circumstance, that they do not, as in health, separate freely and entirely from the serum, but are partly dissolved in that fluid, so as to give it a bright scarlet colour. In the advanced stage the colouring particles become entirely black. Even during life this may sometimes be observed; but after death it is very striking; for on examining together the blood found in the heart, and the matter of black vomit taken from the stomach, it is impossible to discover any difference between them. Another remarkable deviation from the healthy state, invariably remarked, is extreme thinness. This state sometimes presents itself during life, as is shown by the oozing of black fluid blood from the tongue, eyes, skin, or other surfaces. But it is always found in the dead body. It appeared to arise partly from an increased proportion of serum, owing to the stoppage of the various secretions, but partly, too, *Dr Stevens* thinks, from the fibrin not existing in its usual proportion,—being formed in less quantity, and probably also exhausted in supporting the high inflammatory heat at the commencement of the disorder.

These changes in the physical qualities of the blood are owing, as the author believes, to the want of the due proportion of its saline constituents; and some part of the morbid changes are corrected by restoring the proper proportion of saline matter. In this, the theoretical and more important part of his paper, the author reserves the developement of his proofs for a more detailed work which he has projected and is soon to publish. Till then his opinions scarcely lie within the scope of fair criticism. It is not the practice in the present day to receive experimental deductions on the mere assurance of the experimentalist, how eminent soever he may be. On this account, although we do not doubt Dr Stevens has a satisfactory reason for delaying the publication of his researches, we venture to question the propriety of his publishing them now in their present very imperfect form: The short sketch he has given of his views consists, properly speaking, of a train of experimental reasoning, for the several steps of which the experimental foundation is in a great measure, so far as the reader knows, assumed; and in some particulars we may be allowed to suspect from his expressions that his assumptions are actually hypothetical.

We hope the author will take these strictures in good part, and exactly as they are intended. The pathological conditions of the blood and their relations to various diseases, constitute a deeply interesting field of inquiry; and from what we are able to perceive, the particular variety he has undertaken to investigate is very likely to yield satisfactory and important results. But he must not allow himself to be led astray by the warm and deserved encomiums with which his paper has been received in the metropolis, and so be induced to neglect the fundamental inquiries on which his ultimate success must entirely depend. His doctrine appears to be, that in yellow fever the saline matter of the blood becomes gradually more and more sparing in its proportion to the other principles of the blood,—that this is the cause of the blackness and thinness of the blood, which are remarked to increase as the disease advances,—and that the depraved state of the blood, indicated by these changes in properties, in whatever way it may be connected with the origin of the disease, is at all events the source of most of the dangerous symptoms. We have at present but two observations to make on his conclusions, and they relate to the premises on which they are founded. In the first place, care must be taken not to confound the diminution of the saline matter of the blood, arising from mere starving of the blood, and accompanying a relative diminution of all its other solid ingredients —changes which it is well known occur in continued fever, and probably in all acute diseases,—with a diminution of *the proportion* of the saline matter to the other solid ingredients. And, secondly, care must be taken not to fall into the very common mistake of supposing, that the black and dissolved state of the blood, which attends the close, or, what has been appropriately called the stage of agony, of febrile diseases,—and the still more dissolved and black state of the blood in the vessels after death from such diseases,—are substantially the cause of the typhoid symptoms which precede dissolution. This state of the blood, when it occurs during life, is more probably the effect of the typhoid state controlling more or less directly the usual changes which the blood undergoes in its course through the circulation; and when observed after death, it may generally be traced to the particular manner of death,—the gradual or almost imperceptible approach of asphyxia.

Dr Stevens's theory as to the cause of the bad symptoms in yellow fever naturally leads to a very simple method of treatment,—the administration of saline remedies. It was, in fact, the great apparent benefit derived from the use of these remedies, which led him to investigate their action, and to arrive at the theoretical results explained above. The amount of his success is best stated in his own words. "When proper saline remedies are used," says he, "they do not fret the stomach, they act on the intestines as much as is necessary, they keep up all the secretions, particularly that of the kidneys, and enough is absorbed to enter the circulation and prevent the dissolution of the blood, and preserve it until the fever abates, and all the danger is past. This I am warranted to state as a fact, inasmuch as this treatment was commenced in the West Indies in 1827; and since then it has stood the test in several hundred cases of the West India fevers, where it has been tried, both by myself and by others, and with scarcely a single loss when we were called to the patients within the first twenty-four hous after the attack, and with very few deaths where we were called in previously to the commencement of the fatal symptoms." In 1828, when the garrison at Trinidad was in an unhealthy state, the practice here recommended was put in force by Mr Greatrex of the Royals, who reports to the author, "that the above system has been applied to 340 cases, or thereabouts, including both the remittent and yellow fevers admitted into the hospital, after the fever had existed variously from six to seventy-two hours, antecedently to an application to the hospital, with such success, that during the last seven months not a case had died."

This statement is wound up by another which can hardly fail to startle those acquainted with the practice and opinions of other West India physicians. "It can be clearly proved," says Dr Stevens, "that in the West India fevers, *the patients that are left entirely to themselves have a much better chance of recovery than those who are treated with emetics, calomel, or antimony, opium or acids; and that these remedies, instead of being useful, add greatly to the sufferings of the patients; they decidedly increase the very evils they are intended to relieve, and add greatly to the mortality in hot climates.*" If all this be true, *Dr Stevens* cannot be too expeditious in producing his proofs; for we need scarcely remind him how much mischief must be till then daily committed in the West Indies with the remedies he so pointedly denounces.

Case in which a large spoon was swallowed, and produced ulceration of the duodenum and fatal peritonitis, by Mr Houston. (*From the Dublin Hospital Reports,* Vol. v. page 319.)—The body of a maniac who died with symptoms of *peritonitis* in the Richmond Lunatic Asylum was brought to the College of Surgeons for dissection. The peritonæum exhibited all the marks of violent inflammation, viz. copious effusion of lymph, and sero-purulent fluid, with extensive adhesions. A small quantity of the contents of the duodenum was found out of the bowel in the left hypochondre, and a large rusty iron spoon, eleven inches long, was found lying across the spine in the cavity of the bowel with the handle directed foremost and downward, where it had produced ulceration and perforation of the bowel, while the large end was lodged in the *pylorus,* which, though dilated, was neither ulcerated nor broken. The spoon was bent in the centre, where it rested on the vertebræ; but whether it had this form previous to being swallowed, or had acquired it afterwards, it was impossible to ascertain. From the position of the spoon, Mr Houston infers that it had not been swallowed accidentally in the transmission of food, but had been forced down the œsophagus by the insane patient.

Injury of the neck by Suspension. (*Dublin Hospital Reports, Vol. v. p.* 317.)—From the bodies of two felons executed for murder under the combination system, Mr Houston gives the following account of the injuries produced by suspension by the neck till death takes place.

"The cervical vertebræ were unbroken, and the spinal marrow and brain presented no marks of injury. In both subjects the sterno-mastoid on the right side (the opposite to that on which the knot of the rope was applied) were ecchymosed, contused, and broken; the sterno-mastoid muscles on the left side were only slightly bruised. The *os hyoides* and thyroid cartilage were completely severed from each other. The omo-hyoid, sterno-hyoid, and thyro-hyoid muscles were so bruised and lacerated that only some stretched threads of them remained holding the parts together. The thyrohyoid membrane was also torn across, and the epiglottis, pulled from its root at the back of the thyroid cartilage, had passed up with the *os hyoides* and tongue into the back part of the mouth; the skin alone remained unbroken, and interposed between the rope and the cavity of the pharynx. This was the only region of the neck which gave evidence of much injury. All the large vessels and nerves escaped unhurt."

In two other criminals which Mr Houston had a more recent opportunity of examining, precisely the same effects were produced by the rope on the soft parts of the neck, and without any injury to the brain or spinal chord.

Chronic Peritonitis, by Mr W. Law.—A boy, three years old, complained for five months before death; but till the day just previous, he went about, ate freely, and took such stimuli as his medical attendant and those about him considered likely to benefit him.

On dissection, the liver was found obliterated to nearly an ounce weight; the bowels were agglutinated by adhesive matter to each other, and the belly filled with purulent fluid.

THERAPEUTICS.

On the most efficacious mode of applying sinapisms. (Archives Générales de Médecine, Septembre 1830.)—The following observations by *MM. Trousseau and Blanc* will be found useful in settling the proper method of using mustard for sinapisms. They were induced to make some careful experiments on the subject in consequence of the discrepant statements made in works on materia medica. Some recommend that the flour of recently ground mustard should alone be used, others that the bran only of the flour should be taken. Some direct the flour to be made into pulp with warm water, others with vinegar, others with concentrated acetic acid, others with warm water or warm vinegar, indifferently. Some advise that the sinapism be left applied for four hours, others for two or three hours, and others, again, for a single hour only. The authors have very carefully determined the respective advantages of these several practices; and it appears from their experiments, that the several circumstances of difference are very far from being immaterial.

Their experiments were made chiefly with the black mustard commonly used for culinary purposes in Paris. They first found that the flour loses scarcely any of its activity by keeping. In their standard experiment, they remarked, that recently ground mustard, mixed up to a proper consistence with cold water, caused slight smarting in five minutes, and in ten minutes the heat and sense of burning which characterizes the full operation of the sinapism.

They next remarked that flour five months old did not cause smarting within seven minutes, but brought on the full sense of burning in ten minutes, or as soon as the sample of fresh flour. They then found, that when the fresh flour was mixed up with hot, instead of cold water, the smarting commenced in three minutes; but the full sense of burning was not developed in less than ten minutes; and, consequently, although hot water accelerates the commencement of its action, the effect is ultimately and in a very short time the same when cold water is used. The most remarkable results, however, were obtained from their experiments with acetic acid. When the sinapism was made with common vinegar, instead of water, its activity was not increased, as is commonly supposed to be the case; it was, on the contrary, much retarded and diminished. In fifteen minutes, there was extremely slight smarting, which increased so slowly, that in fifty minutes, it was not greater than was produced by the water sinapism in six minutes. The substitution of warm for cold vinegar did not increase its activity. Neither was any advantage gained by substituting for common vinegar concentrated acetic acid, diluted with its volume of water; on the contrary, no effect whatever was then manifested in forty-five minutes. Nay, concentrated acetic acid itself appears not more powerful than simple water. A sinapism made with it had no effect in six minutes; in seven minutes, there was slight smarting; and the full sense of burning was not developed till the twelfth minute. These are interesting facts, because they show that the mustard-flour and the acetic acid possess the property of mutually moderating the rubefacient or irritating properties of one another. Concentrated acetic acid itself seems, from the experiments of the authors, to be one of the most rapid, powerful, and convenient rubefacients that can be employed. When applied on a sponge, or mixed with saw-dust, so as to form a mass like that of a sinapism, acute smarting pain was produced in a minute and a-half; in a minute more, the pain was insupportable; in three minutes from its first application, the experimentalist was compelled to remove it; and notwithstanding the short interval which was thus allowed to pass before its removal, the corrosion produced left a mark three months afterwards. A singular fact, which they have not been able to explain, is, that the activity of a sinapism made with English mustard is not lessened, like that of the French mustard, by substituting vinegar for water.

The following is their account of the several consecutive effects of a sinapism made with water, and of the precautions which should be observed in using it. The first effect is prickling or smarting, which begins in five minutes; in ten minutes, this is converted into acute burning pain, such as is produced by hot iron held near the skin; next, a deeper-seated sensation is felt like that of constriction, or of a heavy weight pressing on the muscles; but after twenty or twenty-five minutes more, the original burning pain returns more violently than ever, and few, who are not insensible from some affection of the brain, can support the sinapism much longer. After its removal, the impression of the cold air relieves the burning for a time; but this, in a few minutes, returns acutely, is soon attended with an uniform, rosy efflorescence, and may continue more or less severely for twelve hours or several days. The authors maintain, that if a sinapism is properly made, and of good materials, it should very rarely remain applied longer than between forty-five minutes and one hour; and that, if it is left for three or four hours, as some direct, its effects may sometimes prove extremely unpleasant. In cases of cerebral oppression during fever, they have known severe sloughing induced by the neglect of this precaution, the physician having been misled by the patient not complaining of pain, and having allowed the sinapism to remain for several hours.

SURGERY.

On Amputation of the Leg at the Knee-joint. (*Archives Générales de Médecine, Septembre* 1830.)—This operation, notwithstanding the efforts of Petit, Brasdor, and Hoin, during last century, to recommend it, has long fallen into nearly total disuse. *M. Velpeau*, the author of the paper of which the following is an analysis, has taken the pains to examine carefully the works of every modern writer of note on operative surgery, and from this inquiry it results, that few mention the operation at all, others pass it over cursorily as having fallen into desuetude, and some even condemn it unreservedly, and assign reasons for not countenancing it. *M. Velpeau*, however, now one of the surgeons of the Hospital of La Pitié in Paris, has lately come to the conclusion, that amputation at the knee-joint is, generally speaking, a simple and safe operation, which terminates in recovery as frequently and as soon as amputation at the middle of the thigh, and leaves the patient a stump as useful as amputation of the leg under the joint.

In his paper he first gives a short sketch of nine cases which have been recorded by authors before him. Of these, one only proved fatal. Of the remainder, the greater number used a wooden leg with great facility, and one of them, a slater by profession, could mount scaffolds and climb roofs, as when he had two legs. He next relates two cases in which he was induced to revive the obsolete operation. The leading features are as follows.

Case I. In January, 1830, a young man of puny frame, and exhausted by diarrhœa and other sufferings connected with necrosis of the tibia, was admitted into the Hospital of St Antoine; and amputation of the leg below the knee-joint was resolved on. When the soft parts, however, were divided immediately under the condyles of the tibia, *Velpeau* found the periosteum lardaceous thickened and detached from the bone, the disease of which evidently extended as high as the joint. Being afraid of alarming his feeble patient by proposing a new amputation in the middle of the thigh, he immediately resolved to amputate at the joint; for which purpose he extended his incisions upwards, and completed the operation without the lad being aware that he had undergone more than one. As there was not a sufficient quantity of sound integuments to form a flap from the back of the leg, an attempt was made to form one out of the integuments in front. But the wound united only in part, and an uncovered space remained about an inch in extent. This, however, was cicatrised in two months; the lad's health and strength returned rapidly; and at the time of the publication of the case he was using a wooden leg as dexterously as if the operation had been performed at the place originally intended.

Case II.—The second instance was a case of severe compound fracture of the left leg, terminating in gangrene. "After a time the mortification, without becoming absolutely defined, nevertheless ceased to make sensible progress. The rest of the limb, though much ecchymosed, instead of swelling more, began to subside in a few days. But an ichorous discharge, which became daily more and more abundant, and was attended with constant, acute pain, an almost continued fever, and diarrhœa, soon dissipated my expectation of preserving his limb. The state of the skin, which could not be saved lower than three finger-breadths below the patella, the ecchymosed condition of the cellular tissue and muscles under the same level, and the extent of the fracture of the bones, which approached close to the joint,—put amputation of the leg under the knee altogether out of the question. Unwilling, however, to amputate at the middle of the thigh, without absolute necessity, I determined to disarticulate the joint, and did so in the following manner. The skin was divided circularly at the level of the tuberosity of the tibia and reflected more than two inches upwards in the usual manner. The joint was then opened anteriorly, just under the patella, and traversed backwards till the knife reached the popliteal artery and nerves; and along with these, the origins of the muscles, were next divided perpendicularly or right across. I thus procured, as it were, a sleeve of integument, three inches in length, which would have rendered immediate union an easy matter, if the traces which appeared of contusion on its inner surface had not compelled me to adopt the opposite mode of dressing the wound." The succeeding fever was high, so that two bloodlettings were required. On the sixth and seventh days erysipelas appeared on the stump, was checked by poultices in the neighbourhood of the wound, but spread up the outside of the thigh and threatened to end in suppuration. Notwithstanding this accident and the subsequent formation of two supparating patches on the angles of the condyles, the wound was quite healed in sixty days. The operation was performed near the end of May, and when the case was published, in the beginning of September, the man could walk with a wooden leg as easily as the former patient.

The details of this case are followed by a brief notice of a third, that of a lad of nineteen, whom *M. Velpeau* had an opportunity of examining very lately, and who had his limb amputated at the knee-joint seven years before. He walked with a wooden leg as well as if the operation had been performed below the joint.

The author next proceeds to consider the different objections that have been brought against this operation. The supposed risk of exposing a large cartilaginous surface to the air he maintains is visionary; the flap may be made to adhere to the surface of the cartilage as well as to other living parts; and though adhesion should not take place, there is nothing peculiarly formidable in the inflammation that ensues. The supposed size of the wound is an erroneous objection, because by his method of operating the greater part of the wound, as in other amputations, will, in general, unite directly. The operation is not, as some allege, more difficult or painful, or recovery more slow than after other amputations. The most important objection,—that the patient is no better off than after amputation in the middle of the thigh,—is also an erroneous one. This might appear sufficiently from the great freedom with which his patients make use of their wooden legs; but it is likewise rendered evident by an attentive consideration of the relative circumstances in which the muscles of the thigh are placed. "After amputation in the middle of the thigh," says M. Velpeau, "how low soever it may have been performed, the point of support for the wooden leg must be the ischium. The movements of the thigh joint are almost annihilated; the gait is the same nearly as if that joint were anchylosed. But after disarticulation of the knee, the point of support is at the extremity of the thigh, the thigh preserves all its movements, and the patient is in the same state as if he had sustained anchylosis of the knee. If it is true, that in respect of the functions of the limb, it is better to amputate under the knee than above it, the advantages of disarticulation of the joint ought to be equally beyond question, for the weight of the body is thrown on the artificial limb after the latter operation exactly as after the former. But besides in disarticulation of the knee the wound is almost entirely confined to the integuments and involves no bone or aponeurosis; the surface to be subsequently covered is convex, regular, free of roughness, and not exposed to the effect of retraction of the muscles: while in amputation at the thigh the wound comprehends an extensive aponeurosis,—innumerable muscles of great bulk,—a bone which is extremely subject to be denuded, and the section of which produces an agitation not always without its danger,—together with the whole cellular tissue which unites all these parts. To conclude, at the knee only one considerable artery must be divided, the twisting or compression of which is almost as certain a mode of preventing hemorrhage as the application of a ligature: while at the thigh, besides the principal artery, there is always a number of secondary branches which require to be secured."

In the conclusion of his paper the author examines the several methods which have been proposed for performing this operation, rejects them all, and describes minutely that which he has found the most convenient. We have described the most material steps of it in relating his second case. The only points which it is necessary to mention here are the following. In dissecting and throwing back the skin care must be taken to preserve on its internal surface all its natural layer of cellular and adipose texture, and the capillary vessels which supply it. Then the patellar ligament being divided, the operator divides the lateral ligaments; and the knee being now slightly bent, he detaches the semilunar cartilages, divides the cruciform ligaments, traverses the joint, and concludes by cutting at one stroke, perpendicularly, and on a level with the reflected skin, the vessels, nerves, and origins of the muscles. Lastly, in adapting the flap, the surgeon arranges the line of union from before backwards so as to correspond with the groove between the condyles; and leaves the ligatures hanging out at the posterior angle.

LEGAL MEDICINE AND MEDICAL STATISTICS.

(*Regulations of the Royal College of Surgeons of Edinburgh.*)— On the 16th of November last the Royal College of Surgeons of this place passed a resolution, enjoining all students who shall, after the 1st of August 1831, commence their curriculum of study for the Diploma of Surgeon, to attend a three months' Course of Lectures on Medical Jurisprudence. This science has at length obtained from most of the bodies which preside over the education of medical students the full attention to which we have long considered it entitled and laboured to procure for it. In the autumn of 1829 the Royal College of Surgeons of Dublin, in the course of last autumn the Society of Apothecaries in London, and now the Royal College of Surgeons of Edinburgh, have all rendered attendance on a course of Lectures on Medical Jurisprudence imperative. A few years earlier it was recommended to the notice of the graduates of this University by an optional statute, which requires candidates for the degree to attend any two of five specified classes, Medical Jurisprudence being one. The Senatus, we hope, will now see the necessity of going a step farther, and adhering to a provisional resolution, passed by an overwhelming majority recently before the enactment of the optional statute,—by which resolution candidates were required to attend this class absolutely. We need scarcely call the attention of the public to the zeal which the Edinburgh College of Surgeons have shown to improve and perfect their scheme of medical education. Notwithstanding the recency of their last extensive changes, a very general feeling prevailed among the members that the subject of Medical Jurisprudence was on that occasion improperly passed over; and it has been now introduced with scarcely any opposition. We have great pleasure in adding that the expectations of the promoters of an improved system of medical study for surgical pupils have not been disappointed, and that the number of surgical pupils continues as great as ever; for the number of inscriptions of first year's students during the session of 1829-30 (the first year when the new regulations passed in 1829 came into operation) was as great as during the previous session.

The London Obstetric Society.—Some months ago, we received from Dr Ramsbothan a letter giving an account of the rise and progress of the Obstetric Society of London, and which, from accidental oversight, was omitted to be noticed, as we intended, in last Number. It was unnecessary to publish the whole letter, which had already appeared in the Medical Gazette, and others of the London journals; and Dr Ramsbothan himself had not expressed the wish for the publication of the whole. We have now, however, to inform our readers, that the formation of a society of this description has been long necessary in the metropolis from the injudicious byelaws and restrictions, as well as the false delicacy of the two London Colleges. It is well known that the effect of these regulations is, that no public cognizance is taken of the qualifications of those who are engaged either wholly or partially in the practice of midwifery. In short, while the present circumstances of society have placed the lives and health of a large proportion of the community in the hands of accoucheurs, London at least is in the singular state of having no public board invested with powers to determine the competency or the incompetency of this now important and numerous body of the medical profession. We conceive it is now too late to talk of the dignity of the profession, and to adduce all the usual arguments by which it is attempted to lower accoucheur practitioners in public estimation. Common sense must suggest that they can neither be better nor worse than others on this account; and their dignity and respectability must depend on their individual conduct and character, and the skill and ability with which they exercise their art. The voice of the public has decided on the necessity of accoucheur practitioners, and they will exist independent of the opinions of colleges.

Under these circumstances, it is obvious that it is quite indispensible to have some authorized board by which the general competency of practitioners in midwifery may be determined, and which may have the power of interfering to prevent unqualified persons from imposing on the public. Under this impression, several of the leading accoucheurs in the metropolis formed themselves into a society for communicating with the two Colleges and the Society of Apothecaries. After repeated applications to these bodies, for the purpose of inducing them to interest themselves in the present state of midwifery practice, the result has been, that, on various accounts, the two colleges did not conceive that it came within the objects of their institution; and the Apothecaries' Society doubted whether their charter gave them any right to interfere. The College of Physicians at length admitted their power of repealing the byelaws respecting practitioners in midwifery, and proposed, "That, as often as any physician offered himself for examination, preparatory to his license to practise, and declared his intention of adding midwifery to the practice of physic, he should be examined before the censor's board by some licensed physician practising, or who has practised, midwifery, who should act as an assessor for the purpose of inquiring into his qualifications in the manual branch of the art."

The society further suggested to the Right Honourable Secretary for the Home Department, the propriety of conceding to the College of Surgeons and the Society of Apothecaries, the authority to examine as to obstetrical qualifications; and at the same time to sanction the introduction of a bill into the House of Commons, rendering it penal for any man to practise midwifery who is not a legally authorized physician, surgeon, or apothecary. The latter purpose was, in the meantime, declined; and the business remains still undetermined.

It is to be hoped, nevertheless, that some definitive arrangement will be made on this point; and that the society will continue to pursue their objects, which are highly laudable, until some positive enactments be made regarding the practice of this important branch of the healing art.

To the Medical Faculty in England.—Cholera Morbus.—The epidemic disorder, known under this name, has of late years committed the most frightful havoc among various nations in Asia. Within the last fifteen months this awful scourge of human life has made its appearance in several provinces of the Russian Empire with undiminished severity. No medical work hitherto published on this fatal malady has been found satisfactory, nor have the suggestions contained in such works succeeded in arresting its devastating course, which, on the contrary, becomes every day more extensive, and seems to threaten the whole of Europe.

Moved by a deep feeling of humanity, the Imperial Government of Russia deem it right to call the attention of the medical profession in Russia, Germany, Hungary, Italy, England, Sweden, and Denmark to this vitally important subject, and to propose to them to forward to the said government treatises on the cholera morbus, which shall embrace the following points.

1st, A clear and detailed account of the nature of the disease.

2d, A statement of the causes which give rise to it.

3d, A description of its mode of propagation.

4th, A demonstration, by the means of exact and faithful experiments, of its being communicable from one individual to another, if such be the fact.

5th, An indication of the measures to be adopted for self-preservation against its contagion should the disease prove contagious; and,

6th, Of the means best calculated to insure a recovery.

Such Treatises or Essays may be written in the Russian, Latin, German, English, or Italian languages, and directed to the "Conseil de Médecine" (Medical Board) at St Petersburg, until the 1–13th September 1831. The name of the author is to be forwarded in a separate envelope.

The author of the best treatise, who shall have fully complied with all the above conditions, will be rewarded by the Imperial Government of Russia with a sum of twenty-five thousand roubles in bank paper, amounting to about eleven hundred pounds Sterling, (L. 1100.)—*Official Notice from the Russian Government.*

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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Sixth Report of the Edinburgh Surgical Hospital, from August* 1830 *to February* 1831. By JAMES SYME, Esq. Fellow of the Royal College of Surgeons London and Edinburgh, and Lecturer on Surgery in Edinburgh.

THE College of Surgeons of Edinburgh having lengthened the course of Clinical Lectures, required as a qualification for obtaining their diploma, to six instead of three months, it has been thought proper to make a corresponding alteration in the period for which the Reports of the Hospital are published.

Since last Report, 925 patients have applied for relief, and 149 have been admitted into the house.

Fractures.—Ever since the institution of the Surgical Hospital, it has been greatly resorted to for relief in cases of fracture both by in and out patients.

During the last month alone, when, it is true, such accidents most abound, from the combined effect of intemperance and frosty weather, the following presented themselves.

Lower Jaw,	1	Patella - -	1
Clavicle,	2	Tibia and Fibula -	4
Humerus,	3	Fibula - -	1
Femur,	3	Metatarsus and Metacarpus	2

It appears that altogether, since the Hospital was opened in May 1829, upwards of 140 cases of fracture have come under treatment. And having thus had a favourable opportunity of observing the circumstances most deserving of attention in the management of these accidents, which can be obtained only in an hospital, and having also made some dissections which tend to throw light on the very interesting and important, though still rather mysterious, question, of the reunion of bones, I think that a part of this Report cannot be better employed than by devoting it to this subject.

Though bones are broken and reunited every day, we hardly ever meet with two surgeons who are agreed as to the process by which their reunion is accomplished. One says that it is ossification of the periosteum; another, that it depends entirely upon effusion from the bones themselves, &c. This diversity of opinion proceeds chiefly from the difficulty of gaining access to the bones while their union is going forwards, so as to ascertain positively the different steps by which it is accomplished; and the best way of putting an end to it is, for every one who meets with an opportunity of making such an examination, to record distinctly and faithfully what he sees. I will endeavour to do this in regard to two cases which lately occurred in the Hospital; but, in the first place, I think it necessary to say a few words respecting the opinions at present entertained on the subject.

The most elaborate and circumstantial detail of the reparation of fractures which we possess, is that contained in the treatise of "Breschet sur la formation du Cal; Paris, 1819." He made many experiments on dogs and pigeons to elucidate the process, and was led to conclude from his observations that it consisted in the following steps. 1*st*, In effusion into the surrounding soft parts, and gradual ossification of a layer of them exterior to the bones; 2*d*, In effusion into the medullary canal, and subsequent ossification of it; 3*d*, The formation of an intermediate substance between the fractured surfaces, which, in course of time, it might be not until months had elapsed, became converted into perfect bone. How far this description is applicable to the changes which take place in the human subject, remains to be proved.

That the bones are not united merely by the ossification of their periosteum, may be proved at once by cutting them through longitudinally, when the ends are found firmly united together, and even the medullary canal filled with osseous matter. If the section, indeed, is made at an early period after the injury has been sustained, the fractured surfaces remain ununited; and hence Duhamel, who, from analogy, had taken up the idea that ossification of the periosteum effected the reparation just as the deposition of wood from the bark unites the graft of a tree, was confirmed in his error, because he did not extend his observations beyond the fifteenth day.

It is confidently maintained by some, and I myself used to subscribe to the same opinion, that the new bone or callus results entirely from the old one, and is gradually completed through successive stages, in which a gelatinous matter effused from the osseous surfaces becomes more and more firm, then cartilaginous, and at last identical with the tissue from whence it proceeded. Analogy, no doubt, is in favour of this explanation; and the appearances observed in bones at a considerable distance of time after they have been fractured also tend to support it; but there are some facts which may be alleged in objection, and, as I think, afford unquestionable evidence against its truth.

It is daily observed, in treating fractures of long bones, such as the tibia and femur, that, notwithstanding the most careful and effectual means are employed to retain the corresponding surfaces *in situ*, they remain moveable for many days, and, indeed, generally for the best part of three weeks, during the whole of which period the crepitation heard or felt by moving the limb is as distinct as immediately after the injury has occurred. The mobility usually ceases very suddenly, and the limb all at once regains such a degree of firmness as to sustain its own weight, or resist any other equivalent force tending to bend it; but if subjected to more considerable violence at this time, it gives way again at the part originally fractured. When such fractures are dissected within the first two or three weeks of their existence, the ends of the bones are found quite separate and unconnected by any intermediate substance. These facts are quite opposed to the idea, that the uniting process consists entirely in the effusion and ossification of a substance proceeding from the surfaces of the bones, in which case the mobility should diminish gradually, and flexibility continue long after perfect mobility had ceased, before the establishment of perfect rigidity.

Case 1.—Catherine Adams, æt. 52, was admitted on the 12th of January, soon after sustaining a fracture of the right thigh-bone in its lower third by falling on her side. Pasteboard splints were applied to keep the limb steady, and then by means of the long splint of Desault, extension was effected, so as to prevent retraction of the broken surfaces, which were very oblique. Every thing appeared to be doing well until the 23d of January, when she had a long and severe rigor, and afterwards complained of general uneasiness, with the other usual symptoms of fever. On the following day, her tongue was brown, and hard; her pulse frequent, but weak; and her appearance upon the whole extremely unpromising. Thinking that she would not bear bleeding, I desired that she should have her bowels freely opened by injections, and afterwards take small doses of an antimonial solution. On the 25th, she complained of her throat being very sore, and her respiration was performed with the peculiar sound which indicates œdema of the glottis. Though this symptom was very distinctly marked, it did not seem to warrant tracheotomy, as there was no indication of any severe degree of obstruction in the breathing, and the patient appeared to be sinking independently of this local disease. I therefore directed blisters to be applied to the throat, and stimulants to be given frequently. She died next day.

On dissection, the fracture was found to extend obliquely from near the middle of the bone down to the external condyle. The muscular fibres and cellular substance in the neighbourhood of the injury were altered in colour as well as consistence, by the effusion of gelatinous matter into their texture. A kind of bag or capsule was thus formed, embracing the whole extent of broken surfaces, and containing two or three ounces of fluid blood. The parietes composing it were in some parts connected to the very edge of the bone, but in others they became adherent to it at a distance of an inch or more from the extremity, leaving a space to this extent uncovered, and apparently denuded of periosteum. When carefully examined, this exposed portion was ascertained to be covered by a thin layer of gelatinous substance, which did not possess the toughness or other characters of a membrane; and the respective surfaces of the bone had a covering of the same kind. The medullary membrane was very vascular, and more distended than usual.

In examining the structure of this bag, I endeavoured to ascertain which of the natural tissues entered into its formation, and in what parts of it, if any, ossification had commenced. On tracing the periosteum from the sound bone, I found that where the bag adhered, that membrane became thick, and evidently continuous with its walls. It seemed probable that where the membrane had been stripped off the bone, as already mentioned, it might assist to form, in some small part, the sac in question; the great extent of which, however, was evidently constituted by the neighbouring tissues, whatever they happened to be, muscle, tendon, fat, or cellular substance, all being reduced to the same appearance internally, by vascularity of the surface, and the same consistence, by the interstitial effusion of organizable matter.

On introducing my finger into the bag, so as to feel if there were any indications of ossification, I perceived some small grains or specks of bone, which, when minutely examined, presented a stellated appearance, and were ascertained to lie in the substance of the capsular membrane. When examined in the same way near its connection with the bone, it was found to contain much larger masses possessing osseous firmness; in order to ascertain the precise seat and origin of which, I carefully dissected the membrane where they existed, and then found that they lay completely imbedded within it, having a covering from it on both sides; also that they did not adhere to the bone, being separated from it by a thin layer of the membrane, so as to admit of a slight degree of motion; but at these parts, the shaft itself had begun to shoot out a growth of new bone.

Case 2.—Mary Donaldson, a poor emaciated old woman, 70 years of age, was admitted on the 27th of September, on account of a compound fracture of the left leg close to the ankle. Both the tibia and fibula were shattered into many fragments, and there was a wound over the latter bone extending down to it. Pasteboard splints were applied, the limb being laid on its outer side with the knee bent; but the patient proved so unmanageable and undocile in favouring the maintenance of steadiness in this position, that I was obliged to have recourse to Macintyre's inclined plane, which answered the purpose perfectly. She made no complaint afterwards, and all her functions were performed in a natural manner. For nearly three weeks crepitus could be distinctly felt when the limb was moved, but then the bones united, the wound healed, and on the 25th of October the cure seemed to be complete. The splints were then removed, and a simple roller applied. On the 5th of November, she was dismissed with the limb perfectly straight.

About ten days afterwards, I was much surprised to learn that she had died in consequence of some internal disease, and having procured permission to dissect the limb that had been fractured, obtained possession of the bones for their more careful examination. When divested of their muscular coverings, they presented an appearance hardly differing from that naturally belonging to them. All the pieces into which they had been broken were firmly united to each other and to their shafts, and were covered with a periosteum of usual consistence. On closer examination, the interstices between these portions were found to be occupied by a soft bloody gelatinous substance, to ascertain the precise extent of which the preparation was macerated. When all the interstitial matter had been thus separated, it was seen that the united fragments of the tibia, which were thirteen in number, constituted merely a skeleton, so to speak of the cylinder, and that the central cavity remained entirely vacant. On examining the internal surface of this imperfect shell, it was evident that an ossific process had been going on over the whole of it, and I have no doubt, that, if the patient had lived some months longer, the bones would have become completely solid. The fibula presented similar appearances, though on a smaller scale, and the process of reunion was more nearly perfected. There is in my possession the preparation of a thigh-bone which was fractured through the neck and trochanters, and was treated by my friend Mr George White. The patient died two months after the accident from some other cause. It now appears, the bone having been macerated, that all the broken portions are firmly united together at the edges, but that all their internal surfaces remain perfectly distinct and separate. The appearance, in short, is very nearly the same, and, I believe, would also have terminated in compact ossification, if the necessary time had been afforded.

I will take the first opportunity of returning to this subject, and endeavour to elucidate it farther. In the meantime, it may not be improper to remark, that the delay which would appear to occur before the broken surfaces of the bones are united, affords no reason or excuse for the practice that is sometimes recommended, of deferring the use of any means to keep the limb in proper position, until ten or twelve days have elapsed after the accident. The first step in the process of reparation seems to be thickening of the surface presented by the surrounding tissues, from effusion into them; and as this, we have reason to believe, is commenced immediately, without a day's delay, if the surgeon defers setting the bones, he will not only lose the assistance thus afforded in preventing their displacement, but also run the risk of allowing the broken extremities to become so far fixed in the distorted situations into which they have been driven by the contraction of the muscles, or the weight of the limb, as to prevent their perfect adjustment by any force which he has it in his power to employ.

Fracture of the Thigh.—There cannot be any doubt, I think, that this accident is most effectually treated by means of pasteboard splints on each side of the thigh, to keep the limb steady, and a long wooden one to make extension. As the muscles of the thigh cannot be relaxed both before and behind at the same time by any position, it is necessary to counteract the retraction which they cause, by the employment of some mechanical extending power; and the simple long splint of Desault effects this completely; the modifications of Boyer and others merely impede its operation. There are some cases that admit of no other treatment; as, for instance, the one mentioned in the former report of a woman, who, from falling out of a window three stories high, fractured both the patella and the femur. On the other hand, it must be allowed, that sometimes there occur cases in which the straight position is inadmissible. I have related one instance of this in the third Report, in the case of a man who had a stiff and bent knee-joint, and may now add another.

Case 3.—Andrew Brown, æt. 44, recommended by Dr Combe of Leith, was admitted on the 25th of September, on account of an oblique fracture of the right thigh-bone, close above the joint. Having had the limb amputated below the knee in India many years ago, he wore a wooden leg, and, happening to make a false step in going down a stair, fell upon his side, so as to make the short end of the machine press on the inner side of the thigh, and fracture the bone, as has just been mentioned.

There was a good deal of distortion, from the lower part of the bone being retracted by the muscles; but we succeeded in keeping it straight by means of pasteboard splints.

Fracture of the Patella.—In this case reunion is opposed by the four following circumstances, 1*st*, The difficulty of retaining the broken surfaces in contact; 2*d*, The absence of vascular parts, or indeed any parts at all on the inner side of the bone, to throw out an effusion of organizable matter for connecting the fragments in the first instance, before they are joined together by their own process of ossification; 3*d*, The presence of the fluid of the joint, which is secreted in greatly increased quantity by the irritation of the injury; 4*th*, The spongy texture of the bone concerned, which is observed to have less power of ossific effusion than the dense portions of the same tissue.

Four cases of this accident have presented themselves to our observation. The first of these was that of a man, æt. 35, from Rothsay, who applied at the Hospital on the 13th of June, immediately after having fractured the patella transversely by falling from a cart. The proper bandages were applied; but he refused to remain in the house, being anxious to return home without delay. The second was of a year or more standing, and the patient, a young woman, came from the neighbourhood of Melrose, anxious to have some operation performed for her relief. On examination, it appeared that the fragments remained perfectly moveable, so that when the limb was extended, they could be placed completely in contact, and upon its being bent, separated to the extent of three or four inches. The limb was consequently very much weakened, and she was therefore desirous of relief. I recommended her to procure a bandage, which should have the effect of retaining the two portions of the patella completely, or nearly, in contact, and this was effected by means of two broad circular collars of leather, which being laced tight round the thigh, were then drawn together by straps at the sides of the knee and patella. She went away very well pleased with the improvement in her condition, and I have heard nothing of her since. The third was also an old case, the patient being Duncan Campbell, æt. 53, from Leith, who was admitted into the Hospital on the 30th of December for fracture of both bones of the leg. The knee presented a very unusual appearance, and upon examination it was found, that there existed not only an old fracture of the patella, but also a subluxation of the joint outwards. These injuries were stated to have been sustained nearly forty years ago in leaping over a high wooden fence. The fragments of the patella were several inches distant from each other, and greatly increased in size; indeed, the upper one was nearly three times as large as it ought to have been. Notwithstanding all this, he had been able to discharge the duties of an infantry soldier for thirty years.

The fourth case was that of George Sinclair, æt. 37, who was admitted on the 1st of January, on account of a transverse fracture of his left patella, which he had sustained the same evening by falling down a stair. The fragments were considerably distant, and a large effusion had taken place into the cavity of the joint. The limb was kept straight, and a pillow placed under the foot, to relax still farther the *rectus* muscle. Discutient lotions were applied to the knee, and circular bandages above and below the patella, with connecting bands on each side of it. The effused fluid was very gradually absorbed, and it was long before the fragments could be brought together. The patient is still in the house, but will soon, I trust, be discharged with little imperfection of the limb.

Fracture of the Scapula.—In one of the former Reports, I related two cases of fractured ileum, and have now to give an instance of the still rarer accident of fracture of the body of the scapula.

Case 4.—John Kevlin, æt. 45, admitted on the 8th of December, soon after suffering the following accident: As he was carrying a heavy stone, in a handbarrow, across the sunk area of a house which was building, the wooden gangway broke, and he, with his companion, were precipitated to the bottom. He being before fell first, and was followed by the stone, which struck him on the back.

Though not prepared to expect a fracture of the body of the scapula, an accident which I had never seen, I at once recognized the existence of such an injury. The lower portion was drawn upwards, and projected outwards by the action of the *teretes* muscles, together with the *latissimus dorsi.* I put a cushion of tow in the axilla, another over the lower part of the scapula, and then applied a spica bandage, under which treatment the patient felt quite easy, and was dismissed free from uneasiness or deformity on the 2d of January.

Fracture of the Lower Jaw.—The lower jaw is usually broken opposite the bicuspid teeth; and Boyer asserts, that it is never fractured at the symphysis. The following case affords nearly, if not altogether, an exception to this rule.

Case 5.—Thomas Reid, æt. 28, received a blow on the lower jaw in the evening of the 12th of January, and in consequence sustained a fracture of the bone, which commenced below the central incisors, and proceeded obliquely backwards on the right side. No means were employed but a simple bandage passing under the chin, and secured over the vertex, and he is now about well. The interposition of any foreign substance between the teeth in such cases is equally useless and annoying; the shape of the respective surfaces presented by the grinders effectually prevents any lateral motion, and the fluid food on which the patient must of course subsist during the cure can be readily imbibed through the insterstices of the teeth.

Dislocations.—In an old woman who died in the Hospital last August, we had an opportunity of dissecting a dislocation of the shoulder-joint, which had remained unreduced for evidently a very long period; the long head of the *biceps* adhered to the groove in which it lies. The lower part of the glenoid cavity had been broken off, and still remained loose, being nourished through its adhesions to the capsule, and a new articulating surface had been formed on the inner side of the neck of the scapula. Four recent cases of dislocated shoulder-joint presented nothing remarkable. One of lateral displacement of the ulna reduced by Mr Nicholson, the house surgeon, was not seen by myself; I can therefore say nothing about it; but think the following case, though it may appear trivial, ought to be mentioned, as I believe it to be of rare occurrence.

Case 6.—Peter Granger, æt. 12, when carrying a pitcher of water fell upon his left hand, and dislocated the first phalanx of his fore-finger from the metacarpal bone backwards. The extremity of the metacarpal bone was felt very distinctly in the palm of the hand, while the finger was bent up and could with difficulty be brought into a straight line with the metacarpus. The hand being held steady, I extended the finger with one hand, and at the same time endeavoured to squeeze the bones into their proper position, which was soon effected with moderate force.

Excision of Joints.—Since last Report, I have cut out four other diseased elbow-joints, making in all fourteen cases of this operation which have occurred to me within the two years which have elapsed since I first performed it. Having already so often and fully detailed all the circumstances of this proceeding, I need now merely notice the peculiarities that were presented by the instances that have just been mentioned.

Case 7. ——— Walker, a boy of about 8 or 9 years of age, was recommended to my care last August, by Dr Fletcher of Dunkeld, on account of an injury of the left elbow, which he had sustained five weeks previously, in consequence of being dragged a quarter of a mile along the road by a cow, to which he happened to get fastened by a rope. The flesh, as Dr Fletcher expressed it, was ground off the elbow to the bones. The limb afterwards assuming a very bad appearance, it seemed necessary either to amputate the limb or cut out the joint.

Finding the portion of olecranon that remained bare, and the joint open, while the limb was greatly swelled, and the patient's health much impaired, I did not hesitate to cut out the articulation in the usual way. The following letter from Dr Fletcher will show how satisfactory the result has proved.

" *Dunkeld*, 14*th December* 1830.

" Dear Sir,—I received yours of the 12th, this morning, and am happy that it has arrived to hasten my doing what I intended to do for the last three or four weeks, viz. to let you know the success of little Walker's case. Until about that time, there was always a slight discharge, sometimes with a scab, sometimes without one; but since then the part has completely dried up; and on examining it to-day, I found the whole surface covered with good sound skin, except one place, about the size of half a sixpence, which has a thin scab upon it, though there is every appearance of its being like the rest in a few days. But while the cure of the wound has been thus successful, the astonishing part of the case is the utility of the arm; its motions are perfect, and it is daily gaining strength; the degree it possesses already is really surprising. To suppose that it will be equal to the other arm is totally out of the question; but its usefulness is more than I could have expected, and certainly a thousand times greater than that of an unseemly stump. If any thing unfavourable should happen in this case hereafter, I shall be sure to write you. I sincerely trust that nothing will go wrong; but if it should, I am confident the cause ought to be ascribed to constitutional fault, and not to the operation.

" Every person who attempts a bold and serious operation, such as the excision of joints, must often meet with vexation and disappointment; but you have every reason for congratulation in the case of Walker.—I remain, Dear Sir, yours most truly,

D. Fletcher."

Case 8.—In last Report, I mentioned that William Finlay, æt. 28, a farm-servant from Cockpen, had applied at the Hospital on account of a small abscess on the inner side of the arm, a little above the elbow, together with considerable swelling and stiffness of the joint. He was dismissed after having the abscess opened and the cautery applied, but returned on the 26th of August in a much worse state than before.

His arm was greatly swelled, and excessively painful. He was entirely deprived of rest, and could not permit the slightest movement of the limb without suffering excruciating agony. The acuteness of his symptoms becoming somewhat mitigated, I performed the operation on the 6th of September, in compliance with his urgent and daily solicitation. The joint was found to be very extensively diseased, the articulating cartilage being entirely detached, except at some part of its edges.

The patient made an excellent recovery, and was dismissed on the 27th of October. He returned lately so much improved in his appearance, that it was difficult to recognize him. The arm is very moveable, and daily increasing in strength.

Case 9.—William Rogers, æt. 13, was brought to me in the beginning of last summer, on account of a diseased elbow, which seemed a very proper subject for excision; but his parents were dissuaded from acceding to this proposal, by the representation of a practitioner adverse to the operation, who strongly recommended amputation in its stead. As this proposal was still less acceptable than the former, the patient was sent to the country, and placed under the care of a veterinary surgeon. I was asked to see him again about the middle of September, when a great change to the worse was observed. The swelling, instead of being circumscribed and confined to the neighbourhood of the joint, now extended half-way up to the shoulder, and down to the hand. There were numerous sinuses, and, in short, an appearance of greater derangement of the structure than I had ever met with, except in the case of Elizabeth Johnstone, which is related in the first Report. Encouraged by the success experienced in that instance, I nevertheless undertook to perform the operation.

The operation was performed on the 21st of September, and proved even more severe than had been anticipated, owing to the ulna being very extensively diseased. The whole of its spongy structure was filled with gelatinous substance, and I therefore cut it across as low as the commencement of the shaft.

The patient seemed to be doing well for nearly a week; but he then lost his appetite, and exhibited the usual signs of irritative fever. Up to the 30th, I entertained hopes of him; but he then began to sink. I then thought it right to give him the chance afforded by removing the limb. He died the day following.

The result of this case shows that there are limits determined by the extent of the disease, and the constitution of the patient, beyond which the operation cannot be pushed with safety. These limits can be ascertained only by experience; and, therefore, exceptions so rare as the one now mentioned should be regarded as beacons, not to warn us against the operation, but rather to guide us in its safe and advantageous performance.

Case 10.—John Nimmo, æt. 12, was admitted on the 1st of October. This boy had been an inmate of the same institution as the other. His complaint had commenced about the same time, and he also had been urgently advised to submit to amputation. The disease was in the left elbow, and presented nearly the same appearance as Rogers' did when I saw him first. I performed the operation in the usual way, and he made a good recovery. He was dismissed on the 27th of October.

Partial Amputation of the Foot.—In the first of these Reports, I detailed the case of a girl Stewart, who had laboured under caries of the tarsal heads of the metatarsal bones for upwards of a year, and had been advised to submit to amputation of the leg, but who was completely relieved by having part of the foot merely removed, according to the plan of Chopart. The result of this case has proved highly satisfactory. The patient having got a very simply constructed artificial foot, she can walk with perfect ease to herself, and hardly any perceptible difference in her gait from that of other people. The apprehension usually entertained in regard to this operation, that the extensor muscles of the heel being unopposed by the flexors, the attachments of which are necessarily all divided, must draw it up, and point the face of the stump to the ground, has, as I expected, turned out to be entirely groundless. The *tibialis anticus* and *extensors* of the toes soon acquired new connections, and enabled the patient not only to prevent the face of the stump from being drawn down, but to bend it upwards at her pleasure. I therefore did not hesitate to resort to the operation in the following case.

Case 11.—William Gemble, æt. 24, a tall, thin, unhealthy-looking young man, of a dark complexion, and dissipated appearance, entered the Hospital on the 31st of August, on account of pain and swelling of the foot. The swelling existed chiefly at the inner or tibial side of the instep, where there was an obscure sense of fluctuation, but also extended across, though not so prominently, to the outer edge. He had observed pain and enlargement for eighteen months, and had used blisters and leeches, but did not experience any considerable pain in it until a few weeks before the time of his admission, since which, he had been confined to the house, and prevented from following his employment, which was that of a printer.

In order to check the progress of the disease, I applied the actual cautery, but without success. An abscess formed at the inner side of the foot, and when it was opened, the tarsal bones were found to be extensively diseased. In these circumstances, I proposed the partial amputation of the foot, but could not prevail upon the patient to submit. He returned home, but came back again on the 1st of September, with his mind made up to the operation.

The foot now presented a much more formidable appearance. Abscesses had opened in the sole, as well as at other parts, and the swelling was greatly increased; still, however, there was no indication that the ankle-joint, or the one between the *astragalus* and *os calcis* was affected, and I therefore performed the operation, as in Stewart's case. The cartilagineus surfaces were found entire, but, as they were somewhat discoloured, I thought it right to cut them off with the pliers.

The patient did pretty well for two days, with the exception of complaining of pain in the stump, and having a very frequent pulse. A very profuse bleeding then ensued, and was arrested by the application of cold and pressure. Next day there was a return of the hemorrhage, and the stump was not only much inflamed, but beginning to slough at the edge of the flap. I carefully extracted all the clots, and introduced a piece caddis between the *os calcis* and flap, which was the part whence the blood issued; graduated compresses and a bandage were then applied, and the case proceeded favourably afterwards, and, though the patient made a tedious recovery, he ultimately got quite well, and was dismissed on the 13th of November.

Urinary Calculus.—In last Report, I mentioned a case of stone that had occurred in private practice, in which I extracted two large stones from a sac formed by dilatation of the membranous part of the urethra. Some uneasiness continued after the operation, which was referred to a stricture of the urethra that had existed previously to it; and from the great relief which followed the introduction of bougies, gradually increased in size, I fully expected that the cure would soon be rendered complete. Finding, however, after the urethra had been dilated to its natural width, that the symptoms were not entirely removed, I passed an instrument into the bladder, and detected a calculus there. The reason of not making this discovery sooner, was, that the stricture being seated in the anterior part of the canal, I had never introduced the bougies farther, from apprehension of disturbing the healing process in the wound of the urethra. Having proposed to remove this remaining portion of the disease, and meeting with great reluctance on the part of one of the parents, I strongly urged the propriety of allowing it to be done, either by myself, or if, as was natural perhaps, inability of understanding the peculiarities of the case occasioned any distrust in me, by another surgeon, who, I understood, had called and inquired for the patient. My remonstrances proved fruitless, and for many months nothing was done; but I am informed that the stone has been since extracted.

This case shows, in the first place, how careful one ought to be in removing stones from the urethra, near the neck of the bladder, to ascertain, at the time, whether or no there are any in the bladder itself. For though it may be of little consequence to the patient in respect either to pain or danger, whether they be taken away together or separately, he will, in the latter case, have all the horrors of two operations, and be ready to listen to any suggestions calling the skill of the surgeon into question.

It is impossible, in such cases, to ascertain the contents of the bladder previously to the operation; because the entrance to it is obstructed by the calculus, and after it is removed, so as to admit an instrument, the truth may still not be discovered, since, as every one knows, a stone frequently escapes detection by the sound at the first trial. The safest plan, therefore, will probably be to divide the prostate so as to admit the finger, and facilitate a complete scrutiny into every corner of the bladder.

Another important fact which it illustrates, is the comparatively small uneasiness generally occasioned by stone in young subjects. This boy, after the concretions were removed from his urethra, walked about as usual, slept undisturbed during the night, and made no complaint, except when he voided his urine. It was this extreme mildness of his symptoms that led me to attribute them entirely to the stricture.

Case 12.—Norman Wilson, æt. 3, recommended by Dr Abercromby, was admitted on the 14th of September, on account of stone, under the symptoms of which he had laboured more than a year.

On introducing a sound, I felt a calculus lying in the prostatic portion of the urethra, and removed it. To avoid a similar mistake to the one just mentioned, I then dilated the neck of the bladder with a bistoury, and introduced my finger, so as to ascertain positively that there was no concretion in the bladder. The boy made a good recovery, and was dismissed quite well twelve days after the operation.

Much about the same time with this last patient, I operated on a young gentleman in Leith, and removed two stones from the bladder with good success. I mention this case merely as it tends to show, in conjunction with the others related in these Reports, that urinary calculus is not so rare in this part of the country as has been alleged. The one I have next to mention is the twelfth that came under my care during the same number of months.

Case 13.—Mr R. æt. 60, from Kelso, entered the hospital on the 26th of December to be operated on for stone in the bladder. He was a man of short stature, but naturally of very stout muscular frame, though now, from great suffering, both of body and mind, reduced to the mere shadow of himself. For nearly three years he had suffered all the symptoms of stone in a degree of extraordinary severity. His cries of agony were almost incessant, both day and night; and to mitigate the intensity of his sufferings, he had so habituated himself to the use of opium as to take latterly sixty-two grains for his daily dose.

I performed the operation on Monday the 21st, and extracted without difficulty a small oval stone about the size of a pigeon's egg. A serious question then presented itself, viz. what allowance of opium should be afforded him? As the irritation which had rendered it necessary was now removed, it might, if continued to the full extent previously used, prove hurtful, while a sudden discontinuance of the accustomed stimulus would be no less likely to do harm. In these circumstances, we thought the safest plan was to leave the patient to the guidance of his own feelings, proceeding upon which, he took each of the first six days from six to eight hundred drops of laudanum. During this period he did as well as could be wished. His pulse never exceeded 62. He recovered his appetite. His urine passed freely from the tube, which was withdrawn on the third day, after which he began to retain it and discharge considerable portions at a time. His bowels were regularly opened by injections, which for a long while had been indispensable to him. I considered him perfectly safe, and he began to talk of the probable time of his return home.

On Saturday, Sir George Ballingall and I thought that he might safely begin to diminish his dose of laudanum. In the evening, I received a message that he was not so well, and on going to the Hospital, found him complaining of exhaustion and general uneasiness, his pulse had increased to 80, and his tongue was dry. As he had taken no laudanum since morning, I prescribed two teaspoonfuls to be taken immediately, and to be repeated every third or fourth hour during the night. On Sunday he was better, but his pulse kept up, and there was a slight degree of mucous rattle in the chest. I directed half the quantity of antimonial wine to be given with his laudanum, and a large blister to be applied to the chest. Dr Abercromby saw him in the evening, and did not think any other measures necessary.

On Monday, Tuesday, and Wednesday, he continued in much the same state, but on the whole became progressively worse. He lost his appetite; his tongue became drier; he complained of great weakness and general uneasiness; and though he expectorated mucus in very large quantity, it seemed to be accumulating in the lungs. He continued the antimony, and had another blister applied to the chest.

On Wednesday, about mid-day, he suddenly complained of a violent pain in the left lumbar region, half way between the spinous process of the ilium and the false ribs; it was not increased by pressure, but remained incessant and excruciating; he cried most piteously, without intermission, from this time forwards, until Thursday evening, when he died.

On dissection, we found the lungs so gorged with mucus that they did not collapse when the chest was opened; they were white and firm, and when divided, still retained their form and appearance. In the abdomen, there was not the slightest appearance of inflammation or other morbid condition, except that the colon, from the middle of its left or descending portion downwards, was extremely contracted, though elsewhere more than usually dilated. This contraction commenced precisely at the part to which the agonizing pain had been referred; but what share it had in its production, I am at a loss to conceive or explain. All the parts concerned in the operation were in the most satisfactory state; there was not the slightest trace of inflammation or effusion in the cellular substance surrounding the neck of the bladder; the mucous coat presented its natural colour; the wound was much contracted in its size throughout, it gradually diminished in width from its external orifice inwards, and was everywhere perfectly smooth and even on its surface.

The fatal result in this case may, I think, with most probability, be ascribed to the effect of suddenly removing a source of extreme irritation in a very irritable system. In ordinary cases of stone, this diminution of irritation constitutes the patient's safety, by counterbalancing the irritating tendency of the operation. But the irritation in this instance being of extraordinary intensity, while the operation, from the small size of the stone, was gently and easily performed, it is conceivable that the actions of the system might, from the cause alleged, fall into disorder, and produce the results that have been described. At all events, I have faithfully related the case, and the practical reader may explain it as he thinks best.

Case 14.—Alexander Mucklereigh, æt. 12, recommended by Dr Cunningham, was admitted on the 28th of January, on account of retention of urine, owing to a calculus in the urethra. He had suffered from urinary complaints for many months, but three or four weeks ago began to experience great difficulty in evacuating the bladder, and at length suffered a complete retention, which had been relieved daily by the introduction of a catheter. A calculus was felt at the bulb, which effectually valved the passage when pushed forwards by the stream of urine, but readily retired backwards under the pressure of the instrument, so as to leave it free.

I could feel the calculus through the integuments a little behind the scrotum, but being unwilling to cut upon it here, where the parts were thick, and from their laxity favourable to the effusion both of blood and urine, I dilated the passage after the manner of the Egyptians, described by Prosper Alpinus; that is, by blowing into it with a tube, having previously, to facilitate the passage of the stone, introduced a little oil. By these means, and making pressure behind the calculus, I brought it forward to the anterior part of the scrotum; but not being able to make it advance any farther, and there being no longer any objection to removing it by incision, I did so, and extracted an oval concretion, about the size of a small plumbstone. The boy made no complaint afterwards. The first time he made water, a little passed through the wound, but none escaped again, and he was dismissed quite well in two or three days.

Case 15.—*Fistula in Perinæo.*—Walter Montgomery, æt 31, recommended by Dr Morison of Dalkeith, was admitted on the 3d of November, on account of a *fistula in perinæo.* I found a stricture at the bulb, and cured it in the usual way, by introducing steel bougies. The urine very soon ceased to pass through the preternatural aperture, except to the extent of a few drops, and issued from the urethra in a full stream; but the external opening, which was much wider than it is in general, showed no disposition to heal. I therefore dilated it freely by incision, and afterwards applied caustic, without any perceptible benefit. The patient then began all at once to cough and spit, with a frequent pulse,—in short, to show all the symptoms of confirmed and rapid phthisis. Though not aware that a connection between *fistula in perinæo* with phthisical complaints is observed similar to that which is supposed to exist between *fistula in ano* and this disease, I suspected that, in the case in question, there might be something of the kind. In the course of two or three weeks it gradually healed, and the patient was dismissed on the 28th of December nearly well, with the exception of his pectoral symptoms, which continued much the same.

Case 16.—*Hæmatocele.*—William Clew, æt. 40, presented himself on the 23d of August, on account of a large swelling of the scrotum, measuring fifteen inches in circumference, and nine in length, which on examination seemed to depend on an enlargement of the contents of the right *tunica vaginalis*. The surface of the tumour was smooth, shining, and of a dark-red colour, it felt soft and doughy, and pitted on pressure, particularly at the lower part. The patient complained of great uneasiness from the weight of the swelling, and the pain which it occasioned when subjected to any kind of motion. He stated, that four years ago he was tapped for hydrocele, and that ever since the scrotum had been larger than before, but gave him no trouble, except from its size, until a month previous to the time of his admission, when, after walking a considerable distance one very warm day, he was seized with great swelling and pain, which suffered no abatement. Shortly before he applied at the Hospital, a small abscess had opened at the bottom of the scrotum.

Having every reason to regard the case as one of hæmatocele, but wishing to ascertain the matter beyond a doubt, I introduced a trocar, and drew off a very small quantity of bloody fluid, the canula becoming obstructed with fibrinous-looking clots, I therefore desired the patient to come into the Hospital, and have the necessary operation performed, viz. incision of the sac, and evacuation of its contents. *Vide* First Quarterly Report.

He was admitted on the 29th; but being then in a very weak and feverish state, I felt unwilling to interfere with the disease until he should get into a better state, the more so as he was rather unusually circumstanced in another respect, having six years ago had the operation of tracheotomy performed, and being unable ever since to suffer the opening to be closed, owing to a permanent contraction of the glottis.

On the 1st of September, the tumour opened spontaneously at its lower part, and discharged a large quantity, twelve ounces by computation, of blood and clots. He was then considerably relieved; but as the great bulk of the swelling still remained, I dilated the puncture which had been made by the trocar, and squeezed out more of the clots, the farther issue of which was then promoted by the application of poultices. The scrotum gradually contracted, and he was dismissed on the 1st of October.

Cancerous Sores of the Face.—It must have been remarked by every practical reader, that when cancerous sores of the face are freely removed by the knife, the wound generally heals very quickly and satisfactorily. This is probably owing to the laxity of the surrounding integuments, as the subjects of such operations are mostly people advanced in life, favouring the contracting effect of the granulating process. Very great freedom may therefore be used in removing these sores, provided it is ascertained that they can be eradicated.

Case 17.—David M'Donald, æt. 65, presented himself on the 12th of November, for a cancer of the right eye. The conjunctival covering of the ball and both eyelids were engaged in the disease, which also extended upwards over the external part of the eyebrow, and downwards for some distance on the cheek. The complaint commenced twenty years ago, and had been constantly extending its ravages, so as at length to attain the extent which has been described, and render him unable to follow his employment, which was that of a street-porter. As the sore did not adhere to the bone at any part, except the external margin of the orbit, which of course could be easily removed along with it, I undertook the operation, and performed it a few days afterwards.

Having circumscribed the whole morbid surface by a circular incision, I dissected away the cancer from the subjacent parts, so far as it was connected to them merely by the ordinary attachments ; but on coming to the external margin of the orbit, I found it necessary to cut away with the pliers all that part of it which bounded the temporal fossa. The whole of the diseased parts, including the eyeball, were then detached, and some pieces of caddis placed upon the wound.

The patient suffered a severe attack of erysipelas of the head and face, but soon recovered from it, and then began to experience benefit from the operation, feeling himself relieved from the burning pain which he had previously suffered in the ulcer. The sore then began to contract, and continued to do so gradually, until a part not so large as a shilling remained to be cicatrized. He was dismissed on the 25th of January much stronger, and looking many years younger, than when he entered the Hospital.

Case 18.—John Niel, æt. 41, from Cupar, was admitted into the Hospital on the 20th of November, on account of a large ulcer which had destroyed the right half of the nose, and more than half of both eyelids. The eye was hardly visible, being pushed away to the side by a hard cauliflower-looking excrescence, which sprung from the morbid surface. The probe being introduced, was readily passed on to the ethmoidal cells and other bones in the neighbourhood. The disease had commenced a great many years before, and had very slowly extended itself. Though this tardy progress was favourable to the success of an operation, in so far as it indicated the absence of any very malignant tendency in the disease, I did not think, in all the circumstances of the case, and especially the affection of bones which could not be removed, that an attempt at excision was warranted, and therefore advised the patient to return home.

Cancer of the Lip.—Ulcers of the lips are sometimes prevented from healing merely by the motion of the part, or the indirect irritation caused by disorder of the digestive organs, in which cases the application of pressure, so as to keep the lip steady, with some metallic wash, such as the solution of sulphate of zinc or calomel and lime-water, and an alterative course of medicine, are sufficient for the cure. But when the sore is truly cancerous—that is to say, seated in a carcinomatous structure, it is to be regarded as incorrigible, and necessarily requiring removal. This may be done either by removing a triangular portion of the lip like the letter V, and uniting the respective surfaces, or by simply taking away the diseased surface with the scissors, and sewing together the skin and mucous lining of the lip.

Case 19.—Mr Pearson, farmer from Kinrosshire, applied a few weeks ago on account of a small superficial ulcer nearly the size of a sixpence, which had resisted the efforts of his attendants in the country, and made him desirous of some radical remedy. I made him apply a piece of caddis moistened with the black wash, protected by a piece of oil silk externally, so as to prevent it from drying, and supported by a bandage, which prevented the lip from moving. In two or three days he returned home with the sore diminished to half its former size, and promising to be soon completely healed.

Case 20.—Jane Dodds, æt. 50, from Earlston, was admitted on the 12th of December, on account of a superficial ulceration of the lower lip, which extended from nearly the middle of it to the right commissure. Its basis was very hard; and, though in some places it presented an innocent appearance, it exhibited in others the usual characters of cancer. It commenced ten years ago, and never healed. A short while before leaving home, she had been taking mercury for this complaint, and having been exposed to cold during the journey, was suffering at the time of her admission from swelled gums, fœtid breath, and the other symptoms of mercurial action. I therefore postponed operating until the 23d, when she seemed to have quite recovered from this derangement.

I then cut away the whole of the diseased surface, but merely the surface, with curved scissors, since the other mode of procedure, which until lately used to be always followed in such cases, viz. that of taking away a triangular portion of the lip, would have been equally unnecessary and injurious. The edges of the wound united satisfactorily, but afterwards separated, as the patient thought, in consequence of a violent cough with which she at this time became affected, and afterwards assumed the characteristic appearance of a mercurial sore. Sulphate of copper and lotions of other metallic salts, with a bandage, were applied, and she was dismissed quite well on the 13th of January.

Case 21.—William Macfarlane, æt. 50, was admitted on the 11th of October, on account of a cancer of the lip, which had existed for upwards of two years. As it was circumscribed and rather deeply seated, while the lip was full, and could afford to lose a portion of it without inconvenience, I made two incisions obliquely downwards, meeting at their lower end, and including a triangular portion. The respective surfaces were united by the twisted suture. He was dismissed on the 15th.

Cancer of the Tongue.—The ligature is usually employed for removing incurable sores of the tongue; but unless the disease is seated very far back, where the hemorrhage, if copious, could not be easily restrained, it is better to use the knife or scissors for the purpose. The operation is thus rendered not only much less painful, but also much more sure in its effect, since a part of the disease is very apt to escape the ligature, however carefully it may have been introduced. If the disease is very deeply rooted, or if there is great tendency in the neighbouring parts to assume the morbid action, even excision will prove unavailing.

Case 21.—Mr M., recommended by Mr Stuart of Kelso, entered the Hospital on the 22d of October, on account of a large ulcerated tumour of the tongue, nearly half of which, both as to length and breadth, was engaged in the disease. It had commenced four months before, and been diminished but not eradicated by the ligature. In order to bring the extent of the disease more completely into view, I removed three of the adjoining teeth, and as it then appeared that the swelling did not descend so deep, or proceed so far back as to render its removal impracticable, I thought it right to make the attempt.

Having seized the diseased mass with a pair of hook-forceps, a very useful instrument on such occasions, which I brought from Paris some years ago, and may now be found in all the cutlers' shops here, I cut it out with a curved knife. In doing this there was no difficulty experienced in taking it away completely from the tongue, but at the floor of the mouth the tumour was found to have deeper and firmer attachments than we expected. After the bulk of the tumour, therefore, had been removed, I dissected out several smaller portions from this situation, and a few days afterwards applied the actual cautery very freely, to destroy any that might still remain.

The wound assumed a very healing appearance, and the patient returned home on the 3d of November, with good hopes of a perfect cure, though I entertained considerable misgivings on the subject. Not long afterwards Mr Stuart informed me that the disease was threatening to return, and I then inquired particularly whether the morbid appearance was confined to the floor of the mouth, or engaged also the tongue. He replied that the whole surface was equally affected, upon which I abandoned all hopes of arresting the disease by operation, and suggested, as the only means that seemed to afford any chance of benefit, the frequent and free application of caustic.

Ranula.—The seat and nature of this disease do not seem to be well ascertained; but the treatment is more determined. A part of the bag ought always to be cut out; and if it is of small size, this generally proves sufficient; but if it is large or of old standing, it is necessary also to introduce a piece of caustic, either potass or nitrate of silver, and touch the inner surface, so as to make it inflame, and either slough or change its secreting action.

Case 23.—*Double Hare-lip, with Malformation of Upper Jaw.*—George Pearson, aged 6, from Stow, was admitted into the Hospital on the 15th of January, on account of a congenital malformation of the lip and jaw. The central part of the jaw, the part apparently corresponding to the intermaxillary bone, projected so as to form a round knob, which bore the two middle incisores, and adhered by a narrow neck to the septum of the nose. On it there lay a small tumour, composed of the integuments which ought to have formed the corresponding portion of the lip. There was no fissure of the palate, but the edges of the lip, which lay on each side of the bony projection, were necessarily at a considerable distance from one another, and not very extensible. Being anxious to preserve, if possible, a complete septum for the nose, which would of course be seriously impaired if the integument resting upon the projection of bone were taken away along with it in the first instance, I resolved to save as much of this skin as might be required to complete the septum, and endeavour to unite it at the same time with the edge of the lip.

With this view, having dissected off the skin, I removed the projection with cutting-pliers. A profuse hemorrhage then ensued, and rendered some delay necessary; as the arteries which spouted were lodged in the bone, and consequently could not be tied. When the bleeding had subsided sufficiently, I made the edges of the lip raw, by cutting off a slice from each; and here it may be right to remark, that the success of the operation for hare-lip, so far as the shape of the lip is concerned, depends much on the mode in which this is done. The edges of the malformed lip being always convex, it is plain that if the surgeon cuts a slice of the same thickness from the whole of their surface, when they are brought together, the result will be an angle at the point of union. But if he cuts so as to make the slice thin at the ends and thick where the edge of the lip is most convex, the appearance will be more natural. I then united the edges together, and having reduced the portion of integument procured from the central part to a proper size for completing the septum, fixed it in its place with a suture.

The child made no complaint after the operation, and the union was subsequently accomplished, notwithstanding some superficial ulceration, owing to the irritation of the needles, and the strain of the stretched lip; to lessen which, a strip of adhesive plaster was applied, so as to draw the cheeks together. There remained, however, a small opening between the lip and septum, over the part of the bone to which the root of the projection had adhered. The surface of this was granulating; and I have no doubt the aperture would either be obliterated, or, at all events, become so small as to occasion hardly any perceptible difference, so soon as the exposed surface of the bone healed. The boy's father being anxious to return home, took him away on the 2d February, before this could be ascertained.

My friend, Mr Dewar of Dunfermline, has related and represented two cases of this disease in the number of this Journal for July last. Having, in the first of these, been disappointed in obtaining union between the edges of the lip when placed together immediately after the removal of the central projection, he advises it to be taken away in the first place, so that all the irritation caused by this part of the operation may have ceased before the uniting process is commenced. The only objection to this mode of proceeding is the necessity which it occasions of leaving the septum deficient, owing to the removal of the skin that covers the projection, which might, if dissected off, be employed to complete the partition between the nostrils. On the whole, I believe the best plan would be, in the first place, to detach the loose corrugated integument that lies on the anterior part of the projection; then to remove the projection itself; next to unite a part of the integument saved, with the septum of the nose, so as to complete it, all of which might be done at one operation; and, lastly, when the patient has recovered from these steps, to unite the edges of the lip. It might perhaps be useful to make the patient wear, for some time previous to the operation, a machine such as that described by Mr Dewar, for pushing the cheeks forwards, and preventing them from being drawn asunder after the operation. This little machine Mr Dewar was kind enough at my request to send to me, and I find it very well calculated for the purpose.—See his description in the number of the Journal above-mentioned.

Wound of Thorax.—Peter Murphy, 8 years of age, was sitting at the door of his father's house, in one of the steep narrow closes in the Old Town of this city, on the evening of the 4th of November, when it was beginning to get dark. Two men, whom he did not recollect to have ever seen before, came walking slowly down, and passing him, one of them suddenly, and without any intimation of his intention, thrust a knife into his breast. He immediately cried out, and the men, after waiting an instant as if to ascertain the extent of his injury, ran off, the one up, the other down the close. His friends coming to his assistance, found a small table or dessert knife, sticking in his side; it was sharp-pointed, and seemed to have been recently sharpened. They immediately took him to the Surgical Hospital.

There was found a small wound between the cartilages of the fifth and sixth ribs of the right side, which evidently penetrated the chest, as bloody froth issued from it during respiration, and his breathing was very laborious.

The edges of the wound having been closed, and a bandage applied round the chest to restrain its motions, twelve ounces of blood were abstracted from the arm, and nauseating doses of antimony were given during the night. Next day his respiration was very laborious, his face flushed, and pulse very quick, and there was a little emphysema round the wound. Eight ounces of blood were taken away, and the antimony directed to be continued. In the evening his breathing was still very bad, and the pain severe; the air-passages seemed to be choked, but he could not spit up any thing. A draught, containing thirty drops of antimonial wine, with fifteen of laudanum, was given, and ordered to be repeated three hours afterwards. At eleven o'clock his breathing was much oppressed; he complained of a severe headach, and his abdomen was tympanitic. A turpentine enema was administered with much relief of these symptoms.

Next morning he spat up a little blood, but his breathing was much easier, though still very quick, and his pulse rapid. The antimony was continued.

He went on improving after this date, and was dismissed with the wound healed, and in all other respects quite well, on the 14th of November.

Wound of Vagina.—Mary Tulloch, æt. 28, was admitted on the 22d of October, on account of an injury which she had sustained a few miles out of town, in consequence of being overturned in a cart. A very profuse bleeding had ensued at the time from the vagina, to the extent, it was reported, of two or three pounds, and when she arrived at the Hospital, there was still great pain, together with tendency to bleeding. A very small breach in the coats of the vagina was observed at the orifice, on its upper surface, round which there was a good deal of ecchymosis. Cloths wet with cold water were applied to the part. A few hours afterwards she complained of pain in the lower part of her abdomen, which increased towards the evening, and about eight o'clock twelve ounces of blood were taken from the arm, and a dozen of leeches applied to the abdomen. An ounce of castor oil was afterwards given to her.

The pain, which had been much relieved by the measures above mentioned, returned next day, though in a slighter degree, and was alleviated by hot fomentations.

On the 25th she felt so weak, that though some pain still remained, it was not considered safe to continue the fomentations.

On the 28th, the pain was rather increased, her pulse was quicker, and she complained of heat, headach, and thirst. The fomentations were resumed, and nauseating doses of tartar emetic were given from time to time.

After this she continued to improve. A small abscess in the vagina, a little above the laceration, opened spontaneously, and discharged a little bloody matter. She gradually got well, and was dismissed on the 13th of November.

It was my intention to have mentioned particularly a number of other cases; but the length to which this report has already extended prevents me from doing so; I will, therefore, conclude by stating the nature and result of the other operations that have been performed.

William Wells, æt. 13, amputation of thigh. Deep-seated abscess. Died.

James Whitehead, æt. 12, amputation of thigh. Caries of thigh-bone. Cured.

—— Fleming, æt. 9, amputation of leg. General disease of the bones of foot. Cured.

Mrs Scott, æt. 65, amputation of the metatarsal bone of the great toe. Caries. Cured.

Marion Montgomery, æt. 8, amputation of great toe. Caries. Cured.

Mrs Miller, amputation of second toe. Tumour. Cured.

Daniel Buchanan, æt. 32, amputation of thumb, metacarpal bone. Injury. Cured.

Thomas Moffat, æt. 69, amputation of thumb. Whitlow. Cured.

Mrs Hird, æt. 24, amputation of finger. Whitlow. Cured.

Margaret Jackson, æt. 49, excision of mamma. Scirrhus. Died.

John Clerk, æt. 80, excision of a cancerous sore of face. Cured.

Margaret Robertson, æt. 22, excision of cancerous sore of arm. Cured.

Alexander Donaldson, æt. 39, extraction of polypus nasi. Cured.

Mary Hall, æt. 25, fistula lachrymalis. Cured.

William Potter, æt. 46, fistula lachrymalis. Cured.

Patrick Safferty, æt. 40, fistula in ano. Cured.

William Glassil, æt. 55, hydrocele. Cured.

William Halliday, æt. 23, hydrocele. Cured.

ART. II.—*Answers to Queries from the Army Medical Board on the Epidemic at Gibraltar* 1828. By R. AMIEL, Surgeon, 12th Regiment, Resident for many years in Gibraltar, and a witness of the Epidemics of 1813, 1814, and 1828.

QUERY *2d.*—What was the civil and military population of the garrison during these years, 1827 and 1828?

Answer.—I believe the civil population of Gibraltar might have been computed at 18,000 during the years 1827 and 1828, and the military at about 5000, including the women and children.

The census taken in 1826 carried the civil population to 15,480, and the one taken in January 1829, at the close of the late epidemic, to 15,470.

Query 3d.—How were the garrison and inhabitants supplied with provisions in each year, plentifully or scantily?

Answer.—The provisions have generally been abundant during these two years, and cheaper than at former periods. The troops were constantly supplied with wholesome food, and with fresh meat three or four times in the week.

Query 4th.—Was there any deficiency of water during these years, in the wells or tanks? Report on the state of the tanks.

Answer.—Water was formerly an article scarce and expensive in this garrison. But since 1814, every house which has been built has had a tank constructed in it. The number of these tanks, which now amount to about 250 or 300, affords a supply of tolerably good water; and we had rain enough during the two above-mentioned years to have prevented a deficiency in this respect; but the poorer classes have seldom access to those tanks, and must purchase water obtained from wells in the garrison, or on the Neutral Ground, which, at the latter part of the summer, is frequently thick and muddy, and at all times rather brackish.

Query 5th.—What vessels arrived at the port during the period of the two years specified, as extracted from the harbour master or health officer's register? Give names of vessels, from whence, and where bound to. Advert to bills of health on board.

Answer.—It has been impossible to obtain the returns called for from the offices mentioned in this query; and all that has come to my knowledge is, that the number of vessels arrived in this bay from the Transatlantic countries, where the yellow fever usually prevails, since the year 1816, to the breaking out of the late epidemic, amounts to eight hundred and forty-four.

Query 6th.—What was the state of the public sewers, and was any marked difference observed in the cleanliness or filth of the streets, houses, &c? Report at large on this subject.

Answer.—I am not sufficiently acquainted with the construction and state of the public sewers in general, to submit correct and satisfactory remarks on this subject; but on referring to an official statement of Mr Woodward's, surveyor of the Revenue Works, I find that the drains in the lower part of the town have but little declivity, and receive the soil and other filth from those in the upper part, into which the privies of the several houses discharge their contents. The consequence is, that, either by direct winds from the west, or eddy winds from the east, the foul air is blown up from the line wall; and in hot weather, the streets and houses are filled with air so offensive and fetid, as frequently to make the people sick.

The whole surface of the drains is covered with night soil, which, from the want of water to clean them, becomes in the warm weather an expanded ridge of rotten matter near the surface of the ground; and the offensive effluvia, disseminated over the whole place, cannot have escaped even superficial observation. During the late epidemic the air was particularly offensive, and great numbers of rats were found dead in the drains. At their outlets on the line-wall, and in many places where Mr W. had occasion to direct their opening, he always noticed that they contained a great deal of filth, particularly at the lower part of the town.

It appears that, previous to the year 1814, there were very few drains in Gibraltar; and, for want of them, large accumulations of filth called *Dirt's-depôts* were established in various parts of the town. In 1815, the reconstruction of the drains took place, and, since that time, they have been continued at various periods up the hill-side; and the gullies have been covered, which undoubtedly is an improvement in the state of the drains. In rainy weather, they have sufficient declivity to be cleared of their contents; but this cannot be the case in dry weather.

In 1828, the drains were much filled with filth, and it may be remembered that there was in the month of July some rain. On the 1st of August heavy showers fell; and about the 17th, 19th, and 20th of August, there was more rain, which brought the contents into action, so that the effluvia arising from them were very like those arising from the dirt deposits in 1814.

As to the state of the drains more particularly belonging to the barracks occupied by the 12th regiment, with the condition of which it has been my duty to make myself acquainted, I can state, that, both in the town-range and King's Bastion, they appear to have been, and continue to be defective. In the lower square of the town range, the drain from the soil-pit was choked up, and burst open a short time before the regiment was sent to camp: and, in the King's Bastion, the sewers at the north and south, probably from a want of sufficient declivity, frequently allow the corrupt substances to accumulate at their entrances, and emit during the summer months exhalations highly offensive, which, in several instances, have been complained of by the men, and reported to the authorities.

The drains about the south end of the building, where the Regimental Hospital is established, are likewise in a very defective state. They have been frequently choked up; and their opening in the kitchen, and in the centre of a very small yard, forms a permanent source of disagreeable, and, I may venture to say, pernicious exhalations. In illustration of this it may be remarked, that the first hospital servant taken ill during the late epidemic was the cook of the establishment who slept in that kitchen, and the disease has been very severe amongst those who have been successively employed on the same duty.

With respect to the cleanliness or filth in the streets or houses, I have not observed any particular difference between this and the preceding years.

Query 7th.—Were you ever in the West Indies or Gibraltar, and have you in either place or elsewhere witnessed a similar visitation of sickness?

Answer.—I have never been in the West Indies; but during my stay in this garrison, I have witnessed the epidemics of 1813, 1814 and 1828, and I likewise had an opportunity of observing a few severe cases of the same fever in 1810.

Query 8th.—In what respect has the late visitation at Gibraltar differed from what you witnessed in the West Indies, or at Gibraltar, or elsewhere, upon any former occasion?

Answer.—The late epidemic has differed from those which I observed before, by attacks more frequently insidious, and more generally fatal in the result.

In 1813 and 1814, the disease, in most cases, set in suddenly with rigors, to which soon succeeded flushings of the face, turgidity of the eyes, darting pains in the head, especially about the orbits, and across the forehead, irritability of the stomach with frequent retchings, and a high degree of fever, which at once indicated a severe and dangerous attack.

During the late epidemic, and especially as the season advanced, the disease has been frequently more mysterious in its approach; and erratic pains about the loins, knees, and calves of the legs, giddiness or slight headach. Eyes dull, with a preternatural dilatation of the pupils; a white tongue; a sense of fulness about the epigastric region; languor; diminution of the muscular power; frequent and deep sighs; partial and momentary moisture of the skin; and frequently but little alteration in the pulse;* were the symptoms which marked its unsuspected progression to that period which characterized the last struggle between life and death.

But, notwithstanding the various shades and modifications of this insidious malady in different years, and in different constitutions, yet the similarity has been sufficient to leave no doubt about the sameness of the affection.

Query 9th.—When and where, (*i. e.*) in what street or house, did you first observe the epidemic at Gibraltar; and was its progress clearly traced from any one point?

Answer.—The first patient of the late epidemic whom I personally saw, was a lance-serjeant (of Company No. I.) quartered at the King's Bastion. He was admitted into the Twelfth Hospital on the 2d September, and died on the 5th. But, from what I have been able to learn, the first cases of this fever had appeared about the latter end of August, in District, No. 24; and this information is completely borne out by the official communication of the late Dr Hennen to his Excellency the Lieutenant-Governor, dated 29th August 1828, an extract of which I take the liberty to subjoin:—

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* The pulse was very often a deceitful diagnostic in the epidemic fever. In several instances I have observed but little variation from the natural standard a few hours before death, when other symptoms indicated approaching dissolution.

"I have the honour to state for your Excellency's information, that, within the present week, I have personally visited, or received reports, of five cases of the bilious autumnal remittent fever, which has made its appearance in a neighbourhood at the back of Hargrave's Parade. One of the cases which I saw had assumed all the appearances of yellow-fever, and proved fatal yesterday; and two of the cases reported to me are stated to be of a similar nature, and will probably lead to a similar event.

"Independent of these cases, which occurred amongst the poorer class of inhabitants, three or four of the family of Mr Martin, chief-clerk in the Civil Secretary's Office, have been attended in a similar disease by Mr Hugh Fraser, surgeon of the Civil Hospital; and it is not a little remarkable, that the fatal case seen by me occurred in a woman who was a servant of Mr Martin. (Vide No. 106, p. 15.)

I have not been able, in any instance, to trace the progress of the disease from a single point; neither did I observe any subsequent attack in the company or regiment, that had any apparent connection with the case of the serjeant whom I mentioned to have been the first sufferer.

Query 10*th*.—Describe generally the symptoms and treatment of a favourable case of this epidemic, which shall have recovered, and place in the appendix three or four such cases as extracted from the register?

Query 11*th*.—Describe generally the symptoms and treatment of an unfavourable case of the epidemic proving fatal, with general appearance after death, and place in appendix three or four fatal cases as extracted from register?

The answers to these two questions are given in two appendixes; but as they do not tend to the direct determination of the question of local origin or importation, it is believed unnecessary to publish them.

Query 12*th*.—Who were the first attacked with epidemic, consistent with your own knowledge, as separated from general report. Were they soldiers, sailors, inhabitants, as merchants or their families, or Jews, Moors, Spanish, or other foreigners? and advert to answer, No. 9. The period when the person had resided on the Rock should be noticed; and if recently arrived from another station, that should be noticed, and its healthiness adverted to?

Answer.—As far as I could ascertain, the first cases of the late epidemic occurred in individuals of the lower class, residing in District, No. 24. My first patient, as already stated in my answer to the 9th query, was a lance-serjeant, who had been four years and a-half in the garrison, and who, I was informed, had been disorderly, and in a state of intoxication, two days previous to his admission into the Hospital.

If I am allowed to cast a retrospective glance on the former epidemics, I may state, that, in 1813, the neighbourhood of Boyd's Buildings [*] was, as in 1804, the first spot on which the disease made its appearance in Gibraltar; and, as early as the 6th of July, a highly suspicious case of fever, which proved rapidly fatal, was met in the person of a ferryman, who lived in that unfortunate situation; but it was on the 10th of September that Dr Gilpin, the principal medical officer, reported officially to the Board of Health, that cases of fever of a very serious type had, within a few days, become prevalent in town, and that masons, porters, bakers, and people exposed to hard labour, had been principally attacked.

The first case of black vomit which I personally observed during that season occurred about the beginning of September, in a gardener who lived at the south end of the South Barracks. He was a Genoese, and had for some time resided in Gibraltar.

In 1814, the cases which first alarmed the garrison occurred about the 18th of August on the hill side, at Cavallero's Buildings, situate close to Arengo's-Gully, and at the top of the central part of the town. These buildings competed at the time with Boyd's for want of cleanliness. They were inhabited by about 300 Portuguese of the lower order, and, close to them, there was an accumulation of filth, (one of the dirt's deposits alluded to in my answer to the 6th query,) which emitted a very offensive stench, and attracted an incredible swarm of flies, which, infecting the whole neigbourhood, became at the time the subject of general observation and surprise.

I may assert, indeed, without fear of the correctness of the assertion being questioned, that, whenever the epidemic has appeared in Gibraltar, it has always commenced in the filthiest spot, among the lower and more disorderly class of inhabitants; and that this was the case in the late visitation, is confirmed by an official communication of the late Dr Hennen to his Excellency the Lieutenant-Governor, an extract of which I beg leave to submit.—(This has been already adduced in Mr Smith's paper at p. 15 of No. 106.)

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[*] Boyd's Buildings covered a space of 29,200 superficial feet, and stood about 200 feet above the level of the sea, in the central part of the town, on a ground which appears originally to have been washed from the mountain down two gullies, at the bottom of which they were situated, and the streams of which they obstructed in the rainy season. These buildings were formerly subdivided into small, dirty, and ill-ventilated tenements, into which individuals of the lower class were generally crowded to an excess, and were considered for a long time as the filthiest spot in Gibraltar. They have since been replaced by roomy and commodious houses, perhaps the best built in the whole garrison.

Query 13*th*.—Do you consider this disease as originating in Gibraltar, or as an imported disease? State the facts on which your opinion is founded.

Answer.—I believe the disease has originated in Gibraltar, and I ground my opinion on the following considerations.

The rise of our epidemic has never been satisfactorily traced to a foreign source, nor its progress marked, from a known focus of contagion, to one or more individuals; and instead of creeping from one family to another, cases have frequently appeared unconnected and scattered at different points; spreading, in some instances, with the rapidity of the electric fluid, and attacking persons who never had approached the sick, nor any assignable source of contagion.*

Our epidemic fever did not spread at Europa Flats, on board of the vessels in the bay, or on the Neutral Ground, when carried thither from the town; and numbers of individuals who had the seeds of the disease about them, even in the case of their falling sick or dying there, did not communicate the malady to their neighbours or attendants. This important fact, which I had particularly noted during the epidemic of 1813, amongst the foreign recruits quartered at the Brewery Barracks, has been most forcibly exemplified during the late calamity, when neither the bedding, clothes, &c. &c. removed from the focus of sickliness, nor the continual intercourse with some of the inhabitants who daily went out to the Neutral Ground, nor the numerous convalescents directly sent thither from the Civil Hospital, often in an early stage of their recovery, produced a solitary case of the disease beyond the gates of the garrison among that numerous population who had fled thither, and who frequently were in confined habitations, when the heat of the weather, the depression of spirits, the melancholy scenes which they witnessed, and, I may add for many of them, privations and hardships, afforded the most favourable means for the propagation of the disease, had it been of a contagious and communicable nature.

Against this assertion it may be stated, that cases of the same fever that prevailed in the garrison, repeatedly occurred on the Neutral Ground, among individuals who, for some time previous to the attack, had not breathed the atmosphere of the Rock. This circumstance certainly leads to the inference, that those individuals contracted the disease by communication with the people going from the town; and this inference is corroborated by the fact related before the Board of Inquiry, that a clergyman whose clothes had been recently stained by black-vomit, having been admitted into a shed, one of the tenants was immediately taken ill, and experienced retchings and febrile symptoms, and that shortly after some of the other individuals in the same shed were similarly affected.

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* Dr Gardiner, surgeon to the Naval Hospital, and member of the Board of Health established in this garrison in 1813, said, in a communication to Dr Burnett, then Physician to the Fleet, "That the disease did not spread from any focus, but broke out in fifty different places at once."

But in answer to this objection, I observe that a contagion producing a disease which manifests itself so suddenly, is yet unknown, and that the sudden attack of the tenant of that shed appears to have been the effect of imagination, rather than of any contagious fomites retained in this gentleman's clothes. This is the more probable, because the disease of the tenant and of the other individuals was mild, and at no time showed an unequivocal character; and because the cases on the Neutral Ground were altogether few, and did not present, as far as I could ascertain, any of the violent and dangerous symptoms observed in the garrison.* The fact, also, that in former years, amongst the few people residing there, probably under 400 individuals, cases of fever were annually observed during the summer and autumnal months, which had been frequently attended with very suspicious symptoms; and compared with which, the few slight cases observed this year, among a population exceeding 6000, placed under the most unfavourable circumstances, far from invalidating my position, evidently tends to establish the principle, that the Gibraltar fever has been literally local, and its infectious properties confined to the vitiated atmosphere of the Rock.

The epidemics which at different periods have raged in this garrison, and on the coast of the Mediterranean, have always appeared about the latter end of summer, or during the autumnal months. If the disease were introduced from abroad, and had the property of propagating itself by contact with the sick, or with substances tainted with the effluvia of the sick, would that property have constantly remained inert during nine months of the year? and do other importable contagions attend to seasons to make their ravages?

The inefficacy of the various means which have been repeatedly employed to stop the progress of the epidemic, such as removing and sending out the sick, shutting up their houses, burning the furniture which they had used, prohibiting intercourse and meetings of all kinds, &c.; and, on the contrary, the success which attended the measure of removing from the impure atmosphere of the Rock those who appeared most susceptible of the fever, as was done in 1813 with many thousand inhabitants; in 1814 with the sickly regiments;* and during the late epidemic, with the whole population of the 13th and 24th Districts, clearly prove that our epidemic fever is not easily removed from its native soil, nor easily transplanted into another place.

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* The case which chiefly excited the attention of the public, and seems to furnish the strongest argument against this, was the death of Raphael Nahon. He had taken refuge on the Neutral Ground in the early part of September, in a shed, with his family, (seventeen in number,) and died there on the 2d of November, after a very short illness. But the annexed certificate of the medical officer who visited him in his last moments, throws a satisfactory light on the nature of his complaint, and removes every suspicion, especially when it is considered that the rest of that numerous family continued to enjoy perfect health.

(Copy.) "*Gibraltar Civil Hospital*, 5th *November* 1828.

"I saw Raphael Nahon on the morning of the 2d of November, in consultation with Dr Foote; the man was moribund; the condition not to be judged of with accuracy. However, if I may venture an opinion, I do not consider the case to have been one of the prevailing epidemic fever, I should rather be inclined to view it as one of serous apoplexy, occurring in a subject naturally constituted of a most gross, bloated, and phlegmatic temperament. (Signed) "Hugh Fraser."

Those cases of fever attended with the same series of symptoms which characterize a severe attack of the epidemic, viz. darting pains in the head, suffusion of the eyes, yellowness of the skin, hemorrhages, hiccup, black vomiting, or dejections, &c. &c. presenting the same morbid appearances after death, and evincing the same property of affording immunity from a second attack, clearly show their identical nature with the epidemical fever, at the same time that their frequent and spontaneous recurrence strongly tends to establish the fact, that the disease originates in Gibraltar, and is produced by local and endemic causes.

An opinion has, indeed, been very prevalent, that the epidemics which have appeared in this garrison, and on the coast of the Peninsula, have constantly been imported in vessels from Vera Cruz, Havanah, or the West India islands. Though this supposition is chiefly useful in explaining its frequent recurrence since the year 1800, it appears to be grounded rather on popular prejudice, and the illusions of terror, than on philosophical investigations. † Besides the difficulties opposed to this mode of introduction by our quarantine regulations, and there can be no doubt of their having been faithfully adhered to, and rigidly enforced in this garrison, the disease has been observed to break out in times when there had been no arrival of vessels from any of those suspicious ports, as it happened in Carthagena in 1811, and in Gibraltar in 1814,[*] and likewise in places where the possibility of such importation could not even exist, as in Medina Sidonia in 1801, a town thirty miles from the coast, where the disease broke out about the latter end of August, when all the sea-port towns in the neighbourhood enjoyed a perfect state of health, and gradually proceeded in the same manner as the epidemics in Gibraltar. If it is difficult to explain that peculiar constitution of the atmosphere which determines the prevalence of fevers at one time more than another, it is equally impossible to account for that contingency which produces influenzas, malignant distempers of the throat, and other mortal epidemics, for a season or term of years, and then allows them to disappear. Moreover, the arrival in this garrison of one or more persons ill of this malignant fever at the breaking out of the epidemic, will not prove its foreign origin or its propagation by contagion; for, on the one hand, this fever has broken out in places where there was not the least possibility of foreign introduction, and, on the other, a number of people labouring under it have sometimes been landed in other places without injury to the health of the inhabitants. It has been alleged by the members of the Sanitary Commission at Cadiz, (Drs Arejula, Ameller, and Cole) that in the summer of 1805, the fleet of Admiral Gravina, on its return from the West Indies, sent to the hospital of that town two hundred sick, most of them yellow, and many with the black vomit, and other symptoms of the yellow fever; *los mas de ellos amarillos y muchos con el vomito negro y otros symptomas de la fiebre amarilla;* that in 1807, the French fleet lying in that harbour landed several patients at the hospital of the Aguada with the same symptoms, where several of them died; and although the vessels from which they proceeded kept up a free and unrestricted communication with the town, the disease did not spread at all; *la enfermedad no se estendio, ni comunico de modo alguno.*[†]

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* In 1814, the regiment of Dillon, which had arrived in the month of May, was quartered, at the breaking out of the epidemic, in the Blue Barracks, near the Moorish Castle. A great number of the men took the fever, and several died, in consequence of which the regiment was encamped on the Neutral Ground, and immediately the fever stopped.

The 8th battalion, 60th regiment, arrived from Cadiz in August of the same year, and encamped on the Governor's meadow in a healthy state; shortly afterwards they went into the Cooperage Barracks in town; the fever soon broke out amongst them, and both the officers and men suffered severely. They were sent back to the encampment, and the disease instantly ceased, as if by magic.

† "In every clime," says the learned Humboldt, "men fancy to derive consolation in the idea, that a disease which is considered pestilential has been brought from abroad. This belief flatters the national pride. To inhabit a country which produces epidemics might be deemed a humiliating circumstance; and it is more satisfactory to consider that the malady is a foreign one, and that its breaking out has been merely the effect of an accident against which it will be easy to guard in another instance. The people immediately adopt this explanation of the origin of the disease, because it is easily comprehended. The medical men, on their side, in general rest satisfied with it, because the word importation relieves them from all responsibility, and from the trouble of investigating the nature and real cause of the disorder. From this has arisen that remarkable facility with which the doctrine of importation has been eagerly received by all classes, when an epidemic manifests itself in a country, and a vessel, a traveller, or a parcel of goods arrive at the same time. So it is that the Havanah, Vera Cruz, and the sea-port towns of the United States, constantly accuse each other of the importation of the yellow fever during the summer months, just as the inhabitants of Egypt refer to the arrival of Greek vessels the appearance of the plague, when in Greece and Constantinople the disease is attributed to vessels coming from Alexandria and Rosetta."

At the commencement of January 1812, when I had charge of the foreign depot, about 460 foreign recruits arrived in this bay from the Spanish coast; part of them were landed here on the 13th of that month; but they were re-embarked the next day on board of the *Downs* and *Langley* transports, to proceed immediately for England. By some accident the Downs lost the convoy in the night, and came back to this bay, where she lay on the 28th, when I was desired to go on board, as some of the recruits had been taken ill. I observed that, besides their being in a crowded state, they were in great want of clothes,—a circumstance which induced them to stay constantly between decks, in order to avoid the inclemency of the weather. On the 30th of January, the sick, the number of which had increased to thirteen, were landed and brought to the Hospital of Foreign Recruits; and on the 2d of February, eighteen more of the same men who had taken ill were likewise landed, and sent to the Hospital of the 7th R. V. B. The number of the sick, however, continuing to increase among them, an hospital was established afloat on board the transport *Edward*, and, I believe, not fewer than 40 of the same men were admitted into it in the course of a few days. The fever in all these men presented a type more or less continued; was attended with irritability of the stomach, bilious vomitings, yellowness of the skin, &c. &c.; and, although of a milder nature, probably on account of the cold season of the year, had in all essential points a perfect resemblance to the epidemic which has prevailed in this garrison.

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[*] In 1814, the first alarming cases of fever occurred about the 18th of August, and the last vessel which had arrived from Havanah was the Spanish Polaca Santa Secla. She arrived in this bay on the 17th of July in a healthy state, and sailed the next day for Alicant; and I never heard of her having been suspected at any time, either by the people or the authorities, to have left the seeds of the fever which broke out one month after in this garrison.

[†] Vide dictamen de los tres professores Medicos Comissionadis pro la Suprema Junta Sanidad para in dagar el origen de la calentura reynante, 1810, page 2.

The circumstances which preceded the breaking out of this fever render it impossible to trace it to the introduction of a foreign poison; and its not having spread to the people lying in the same wards with those patients when ashore, proves that it was not propagated by a specific contagion, but was a disease generated in that filthy and crowded vessel—which is the more credible, as a ship, having many places inaccessible to ventilation, and where corrupt substances may be concealed, will easily become an unhealthy and dangerous place of abode, from the crowded state in which people may happen to be on board, from inattention to cleanliness, from the bad quality of provisions, &c. &c.; and there are but too frequent records in the annals of medicine of the yellow fever having broken out in vessels when passing into tropical latitudes, or lying in our harbours during the warm seasons. Evidence of these facts is afforded by the history of the English ship Penelope taking emigrants in 1801 from Ireland to New York; by that of the Hibbert from Portsmouth, bound to Honduras in 1803; and of a vessel from Liverpool to Demerara, with horses on board, as mentioned in a letter from Dr Chisholm in the Medical Repository, Volume v. page 229; and also by the statements of Drs Meyer, Rolland, Legier, Pineda, and Peres, quoted in the Report of the Commission appointed in 1828 by the French Royal Academy of Medicine of Paris, to examine Dr Chervin's documents concerning the yellow fever. In fact, whether an epidemic fever arises from filth and putrid vapours ashore or on board, the cause and accompanying circumstances are the same, and must lead to the same result.

Query 14th.—In what respects does the late epidemic differ from the bilious remittent fever of the same place at certain seasons of the year?

Answer.—I believe the epidemic differs only in degrees of intensity from the autumnal bilious remittent fever of this climate; and I am the more strongly induced to adopt this opinion, as I have occasionally witnessed, in times when the bilious remittents were frequent, cases attended with the violent attack, the high malignity; and the rapid dissolution which more generally mark the character of the epidemic; and during the epidemic season, instances have been frequent in which the disease has retained the mild form and the distinct character of the bilious remittent. * This was particularly remarked in 1810, and during the epidemic of 1814, and has also been remarked at the breaking out of every epidemic, probably from the circumstance of the morbific causes not having yet acquired their concentrated energy and virulence.

I was in this garrison in the year 1810, and I had the opportunity more particularly to observe the different gradations of the disease as it appeared in this place during that year. Bilious remittents were very prevalent in July and August, especially in the hospital of the foreign recruits, of which I had the charge. In the month of September some cases of that fever appeared likewise in the same hospital, but they were attended with more malignity, and two ended fatally with hemorrhages from the nose and mouth. I was at that time desired now and then to visit some inhabitants labouring under a fever presenting the same symptoms; especially in the neighbourhood of *Scud-Hill*. On the 20th October, I was called to see Jacinto Reys, a carpenter in the dock-yard, attacked with fever. He had been working three or four days on board the *San Juan*, lying in the Mole, where he was taken ill. The leading symptoms were severe headach; eyes injected; dorsal pains; pulse very quick, and full; great irritability of the stomach; constant retchings, and vomitings of bilious-like matter, &c. &c. The disease went on through its periods to the seventh day, when free perspiration was followed by a favourable crisis. On the 22d of October, Andrew Reys, an elder brother of the patient, who lived in a neighbouring house, and had been in the habit of visiting him frequently, was taken ill with the same leading symptoms; and in the course of three days, the mother, the grandmother, a child, and a person who lived on the same floor, were taken ill of a fever presenting the same symptoms as Jacinto Reys. Andrew Reys died of that fever on the 26th of October; the rest of the family were sent to perform quarantine on the Neutral Ground, and sentries were placed around the houses they had left, to prevent any intercourse of the inhabitants with that spot.

No new cases appearing in the garrison, † the quarantine was taken off on the 24th November, and the families allowed to return to their homes. A younger brother, however, named Joseph Reys, fourteen years old, who had been very healthy on the Neutral Ground, was, three days after returning to his house, seized with a fever which presented the same symptoms as his brothers, but in a more aggravated form. On the third day a vomiting of a black matter came on, and he died on the fourth day from the attack.

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* Dr Arejula, in his *Succinta Descriptio febris Epidemicæ Malagæ* says, that in 1803 the febrile actions frequently assumed a remittent type, the cold, hot, and sweating stages being evidently distinct, and the disease was frequently protracted to the 11th, and sometimes to the 14th day.

† A considerable change in the state of the atmosphere took suddenly place on the 27th and 28th October of that year, in consequence of heavy showers of rain and

The circumstance of that boy having been attacked with fever soon after his return into the garrison, was at the time brought forward as a strong argument of his having caught the fever by coming in contact with the clothes which had been used by the family when sick, and before they had been removed to the Neutral Ground; but, besides this having been a very gratuitous supposition, which the family denies, the circumstance of two Genoese, Santiago and Francisco Prospero, having been taken ill in the neighbourhood, and on the same day, (the one of whom died likewise with black vomit,) without having had any communication with the Reys's family, disproved such assertion; and considering that the small district where those fevers broke out was then so filthy, crowded, and unventilated, as to have induced the Lieutenant-Governor to order nine sheds to be removed from a space of ground not exceeding 3000 superficial feet, it appears highly probable, that the disease had originated in that spot, from local and domestic causes.

On examining with attention the circumstances of the fever which prevailed here during that year, it appears to me evident, from the facts which directly came under my observation, that atmospheric causes, more or less powerful, according to the different changes of seasons, gradually influenced the degree of intensity of those fevers, from the mild type of the bilious remittent in July and August, to a more aggravated one in September and October, attended with great irritability of the stomach, but with remissions still distinct, and lastly, in November, to that malignant type which brought on the dissolution in four days, preceded by black vomiting. The causes of the fever which attacked the whole of the unfortunate Reys's family must have had the same origin; but those causes were more concentrated, and had acquired a greater energy when they attacked Joseph Reys, the last victim.

Bilious remittent fevers were likewise very prevalent during the summer of 1813, appearing under the same mild type in July and August; but, as the autumn approached, I observed they were assuming a more aggravated character, and at the commencement of September, two gardeners died in the south, one opposite Cooper's

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north winds. The thermometer, which had until then ranged from 65 to 78, rapidly sunk to 53,—a degree of cold, (says Dr Pym in his official communication to the Army Medical Board, dated 2d November 1810,) very uncommon in this climate at this season of the year, and, in his opinion, very favourable for the destruction of the contagious power of the disease.

Buildings, and the other near the South Barracks, in less than five days; and these two fatal cases, which I know to have been previous to, or contemporary with, those observed in City Mill Lane, have convinced me that the report which was then circulated, of the fever having been imported from Cadiz, in the latter place, was entirely groundless, and the history of the epidemic of 1810, 1814, and 1828, showing likewise a domestic origin, corroborates the opinion, that the bilious remittent and the epidemic are modified gradations of the same disease, and proceed from the same sources;—that they are a disease of the climate, equally distinct from the continued fevers of crowded cities, as from typhus, which is known especially to arise from an accumulation of human effluvia.

In support of this position, I take the liberty of annexing an extract of the summary remarks of the late Dr Hennen on the diseases of the quarter ending 20th September 1826; and the observations of such an able observer, equally distinguished by sagacity and judgment, cannot fail to have great weight, and in my opinion are conclusive.

"Some of the fever cases required very active treatment, and were attended with considerable determination of blood to the head or abdomen. These were principally occasioned by irregularity of living, and exposure to the heat of the sun. In other cases the attack was of a less marked character, and in a few made its approaches in a very insidious manner, developing in its progress all the characters of the autumnal bilious remittent, the endemic disease of the whole of the Mediterranean coast. One of the cases occurred in a man who had been for a long time employed on the Neutral Ground, and three took place in individuals who contracted their disease within the walls of the garrison.

"Upon the whole, although the Neutral Ground principally is productive of fever of this description in the autumnal season, I cannot entertain a doubt that the disease also appears among persons who have contracted it in town, having seen no less than six cases, including civilians, in proof of it, within the last three months; in two of these, dark-coloured matters were ejected from the stomach resembling coffee-grounds, and, like them, sinking in water. In those cases, dissection exhibited a highly diseased state of the stomach, the coats of which were inflamed, pulpy, and as easily torn as a piece of blotting-paper. The practice which I am led almost invariably to recommend in these fevers, is the early employment of calomel, so as to affect the mouth, aided by an equal early use of the lancet, or of leeches, according as circumstances may indicate general or local bleeding; to which are added tepid spongings, purgatives, &c. &c. as occasional symptoms may demand. I have never witnessed more striking relief in any disease than was produced by the application of fifty leeches to the head and temples of an officer's wife whom I attended. Her disease was contracted upon the Neutral Ground, where her husband has long resided as commandant. Another inhabitant who lived in the gardens on the opposite side of the road was not equally lucky. He died on the fourth day, harassed by incessant vomiting of the nature mentioned above, and with his skin deeply tinged with yellow.

"Six out of the nine deaths which occurred during the quarter were the consequence of fever."

Query 15*th.*—What the diagnostic symptoms?

Answer.—The epidemic fever, as far as my knowledge goes, has no specific character, no appropriate, essential, or pathognomonic symptom, no definite course of duration. But its prominent features are most generally great pain in the head and in the loins; strong and quick pulse; turgidity of the vessels of the conjunctiva; moral and physical prostration; sensibility and irritability of the epigastric region; anxiety; tongue dry and red; yellow tinge of the skin; great variations in the urinary secretions; passive hemorrhages from the nose, tongue, gums, anus, &c.; dark and even black vomitings or dejections; delirium; coma, &c. The whole or part of these symptoms appearing together, form what I consider the surest diagnostic of the epidemic fever.

Query 16*th.*—Do you consider the disease contagious, *i. e.* propagated by contact; and did the very latest cases appear to have been contagious?

Answer.—I do not think that the disease has been at any time propagated by contact; and although the facts stated in different paragraphs of my answers in support of this opinion are merely negatives, they are general facts, so constant, and so repeatedly observed, that, altogether, they afford what I believe most conclusive evidence.

Query 17*th.*—Do you consider the disease infectious, *i. e.* propagated by the infected atmosphere of a sick room?

Answer.—I am not prepared to say how far the atmosphere of a sick room may, by the accumulated effluvia from numerous bodies labouring under a violent form of the epidemic, contribute to the propagation of the disease; but I believe that a man affected with this fever will not communicate it to another sleeping in the same room, when there exists no epidemic influence, and where common cleanliness is observed; and I ground this opinion upon the following facts, which came directly under my observation during the late epidemic.

It is a well known fact that the fever spread in the South two or three weeks later than in the town. From the 2d of September to the 1st of October, several cases of the epidemic were admitted into the Regimental Hospital, three of which died with the black vomit; but the disease never attacked any of the other patients, (more than twenty in number,) treated during the above period for other complaints, nor any of the orderlies, who had, as usual, an incessant and unreserved intercourse with the dying, and slept in the same wards. It was only on the 25th of September, when the disease had spread in the south, and when the epidemic influence had extended to the district where the hospital is situated, that the cook of the establishment, who never had occasion to approach the sick, contracted the disease; and in the month of October, when the atmospheric causes had acquired more intensity, the hospital serjeant and twelve orderlies, successively sent from the camp, were taken ill but a few days after entering the precincts of the hospital; and several of them fell victims to the disease.

Query 18*th*.—Do you consider the disease epidemic propagated by the general vitiation of the atmosphere of the Rock?

Answer.—That our epidemics are the result of an atmospherical vitiation appears to me evident from the fact of the disease having been constantly influenced by atmospheric circumstances. It has had a fixed period for its appearance, a fixed period to attain its maximum of mortality, and a fixed period for its termination. If this last epidemic has been protracted two or three weeks beyond its regular duration, it is to the influence of the mild temperature which we enjoyed during the latter part of the season, that this circumstance must be ascribed.

The different epidemics which have appeared in this garrison at different periods, as well as those which have raged at Cadiz, Xeres, Sevilla, Malaga, Alicant, Carthagena, Barcelona; and perhaps I may add others which have appeared in similar latitudes, as in New York and other towns in the North American coast, have constantly broke out about the latter end of August or beginning of September; they have gradually increased so as uniformly to produce the greatest mortality in October, most generally from the 9th to the 20th,* and then to diminish successively until the noxious exhalations have been condensed by cold, or washed away by heavy rains.

The numerous instances of two, three, or more persons having been attacked in the same house, at the same time, and even at the same hour; and that general susceptibility, which so rapidly prevades all ranks, show, not that the disease has the property of spreading from person to person, but rather that it is produced by a general cause, to the influence of which they have been simultaneously exposed.

If this disease were produced by any other cause than an atmospherical vitiation, would the period when the greatest number of patients affords more extensive means for its propagation in populous cities, become the marked period for the abatement of the disease, and for the diminution of the number of its victims? In

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* In Sevilla, the greatest mortality, in 1800, took place on the 14th of October, and in the epidemic of 1804, on the 9th of October, at Gibraltar, Cadiz, Alicant. On the 10th, at Antiquera and Grenada. On the 15th, at Xeres, Cordoba, Montilla, Moran. In 1821, on the 19th of October at Barcelona; and in Gibraltar in the epidemic of 1813 and 1828, the greatest mortality occurred on the 17th of October.

1803, the population of Malaga, at the breaking out of the epidemic, amounted, according to Dr Arejula's Tables, to 48,015, and out of that number, 16,517 were attacked, and on the following year 17,000. In Barcelona in 1821, the population which remained in town, was computed to about 80,000, 30,000 of which passed the disease. And in Cadiz in 1810, out of 130,000, 12,000 were attacked; but in Sevilla, in the year 1819, the number of those who contracted the disease did not reach 2000 in a population exceeding 70,000 inhabitants.* Thus the disease, in all these instances, did not stop its progress for want of subjects, or by the effects of any sanitary regulations, but evidently began to subside when the atmospheric agents diminished, or ceased from their deleterious influence.

If this disease were connected with any other cause than an atmospherical poison, would its ravages be constantly limited to certain climates, in these climates only to certain places, in these places only to certain seasons of the year, and even in these seasons would it only appear after a particular degree of heat and moisture?

The peculiarity in our epidemic of modifying, or completely changing the nature of all acute, and some of the chronic, diseases, drawing them, if I may be allowed the expression, into its vortex, and stamping them as it were with its own seal, affords, in my opinion, a convincing proof that its primary cause must be an atmospherical agent, as no other in nature could so uniformly extend its influence upon all who lived in a district, or a town, &c. &c. so as to produce the general, sudden, and uniform transition from the healthy to the unhealthy state, which are remarked during the prevalence of the epidemic; and further, as the alterations in the atmosphere happen generally at regular seasons, so the disease connected with these variations have constantly appeared at regular periods. Thus it is that a scientific observer well known in the annals of military medicine (Sir John Pringle) who has constantly considered the degree of heat, at the latter part of the summer or beginning of the autumn, joined to a dampness of the atmosphere, as the cause of the bilious fever of camps, refers to the same source the bilious fever of every country and of every climate, as well as the yellow fever of America; which, in his opinion, is no other than the maximum of the bilious fever. In fact, the connection of the yellow fever with the state of the atmosphere is so intimate, and so well marked in the intertropical regions, that another accurate observer, (Dalmas,) after a residence of ten years in the West Indies, could, by the inspection of his barometer, foretel the reappearance or cessation of this malady.

The instances of people labouring under this fever having had intercourse with others liable to it on the Neutral Ground, or at Europa-Point, are very numerous; but it is very doubtful if in any instance the disease has been contracted beyond the sphere of the local causes from which it has originated.

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* Vide Relacion do lo occurido en Sevilla con motivo de la enfermedad de 1819, page 14.

The 12th regiment was removed to the Neutral Ground on the 5th of September last, after having sent five cases of fever to the Twelfth Hospital, two of which died with the dreadful symptom of the black vomit; but, from the period of their encampment until the 25th of September, the soldiers having had no duty to perform in the town, no fever cases appeared amongst those in the camp, while several orderlies attached to the different departments who continued to reside in the garrison were attacked with the epidemic; and it was from the 25th September, when the regiment had resumed the town duties, that is to say, when the soldiers came to breathe the pestilential exhalations in their stationary sources, that the admissions became numerous, and the increase of disease most alarming.

Ninety-two women of that regiment, and 190 children, who never were allowed to repass Bayside barrier, have continued perfectly healthy; and one woman only, (the armourer's wife) who, during the period, obtained leave to enter and stay a few days in the garrison, caught the fever and died of it.

Several of these women passed the night in the same beds with their husbands attacked with, and labouring under, the epidemic fever, and besides continued, as well as their numerous children, to use the same bedding after the men had been removed to hospital; but in no instance was the disease contracted by the wife or the children, even after that full exposure.

Four orderlies employed in the hospital sheds on the Glacis, who attended several cases of fever admitted into that hospital, escaped the disease; while the whole of those employed during the month of October in the hospital at the south, one of the watermen excepted, contracted the epidemic, and several died.

It has been said that the pure air of the Neutral Ground checks the contagious property of the fever; but when the wife in the same bed came in contact with the patient scorched with febrile heat, or bedewed with copious perspiration, when she inhaled under the same tent the effluvia of his breath, how could the air, however pure, sufficiently interpose to arrest the process of contagion, and its fatal consequences?

From the foregoing observations, I conceive it results that the disease does not spread when the sick are removed from the impure air in which it was contracted, and likewise that, by breathing the impure air without exposure to the contact or effluvia of the sick, persons are frequently attacked; while, on the contrary, without breathing it, however exposed to such contact or effluvia, no person is attacked. Experience, therefore, warrants the conclusion, that impure air is the primary and essential cause of this disease.

I consider as secondary causes mental depression, exposure to heat, cold, fear, intemperance, anxiety, &c. which, in some individuals, have brought on an immediate attack of the epidemic fever; while others, by carefully avoiding these exciting causes, have been uninfluenced by the noxiousness of the atmosphere, and escaped the disease.

Query 19th.—Each opinion to be supported by facts without any reasoning?

Answer.—The facts on which my opinion is founded are described in my answer to the preceding query. I may add, as an observation in some manner forced on me by the circumstances attending the same, that, when so many new cases occurred in the regiment after it had resumed the town duties, the men composing the guards were marched directly from the Neutral Ground to the guard-houses, mostly by the Line-wall, and avoiding the streets as far as possible; that, in those guard-houses which were frequently white-washed, they had no communication whatever with any description of inhabitants, and still less with the sick, or articles belonging to them; that they were marched to and from the posts of the guards without being allowed any improper intercourse; and that, ultimately, they went back to the Neutral Ground with the same precautions, and in the same orderly manner as they had come from it; thus rendering it impossible to trace to any contagious source the malady which unexpectedly manifested itself in many of them but a few days after they had been employed on that duty. And, indeed, to what sources could be referred the frequent and violent attacks which continued to be more especially observed amongst the soldiers at the North-Flat Bastion Guard, which is situated in the 24th District, after all its inhabitants had been removed, and the houses shut up, but to the deleterious exhalations, which, having produced on that spot the first cases of the disease, successively formed a focus of infection so fatal to the soldiers who breathed the vitiated atmosphere, that government found it necessary to suppress that guard, and even to remove to a greater distance that which had hitherto been stationed at South-Port Gate, the nearest to that infected district.

Query 20th.—Had malaria, or the effluvia from marshy places, any, and what, influence in the production of this disease?

Answer.—From a careful attention of the different epidemics and sporadic cases of this fever which have come under my observation during a residence of eighteen years in this garrison, I am induced to consider putrid effluvia as its primary and essential source. Like remittent fever, and all diseases known to arise from the same cause, the epidemic has never appeared but in the warm season, when heat, acting on putrid vegetable and animal matters, produces the febrilizing gases; and, like them, it has ceased when the sources of those morbific gases have been condensed by cold, or washed away by long-continued rains. Like them also, it seems to have exerted its morbid influence more particularly on the gastro-hepatic system, and has been marked by that same characteristic diversity in its mode of approach and individual attack, which, according to the less or greater intensity of the exciting causes, resembles a mild bilious fever in some, and the rapid effects of a deadly poison in others; thus evincing, by the nature of those variations, that the epidemic having no peculiar symptom to fix its character, differs from the diseases propagated by a specific virus, and is evidently the offspring of those effluvia, which, in their mysterious combinations, as they are more or less virulent and concentrated, produce an ague in the plains of Lombardy, and a most malignant fever on the Banks of Senegal.

But an argument which farther establishes the influence of malaria in the production of our epidemic, may be derived from another peculiarity known to belong to all malarious diseases,—namely, that its attacks were more dangerous, and have been more frequently brought on by exposure during the night. This observation was too often confirmed during the late calamity, by the frequent attacks amongst the soldiers sent from camp to perform night duties in the garrison, when numerous inhabitants who continued their intercourse, as in 1814, came every morning from the Neutral Ground to their daily avocations in the town, have most generally escaped the disease, by avoiding to pass the night in the fortress.

It is true that the sources of marsh miasmata are not easily traced in Gibraltar, where the Rock is covered with soil only to a certain distance from its basis, and the declivity in general so rapid, as to render it very difficult for any water to stagnate, or to remain long on its surface.* But the vicinity of an extensive beach, where, amongst other nuisances, all the drains of a numerous population empty themselves, the nature of the soil chiefly formed by the alluvial matter of the rock, the decay of vegetable productions in the several gardens covered with rich manure, and the neglected state of the privies and drains, in spite of the wisest regulations and unremitted exertions on the part of the authorities, may, under a powerful sun, and a degree of moisture which easterly winds, fogs, and slight rains impart to the atmosphere, produce in the latter part of the summer exhalations as deleterious as marsh-miasmata, which, according to their degree of concentration, and the predisposing circumstances, will generate fevers more or less aggravated in their type or duration.†

That Gibraltar has at all times possessed within itself many local causes of fever capable of assuming the most malignant type when exercising their full influence upon a densely crowded and filthy population, is perfectly obvious, and established beyond the possibility of cavil, by the records of the Civil Hospital, and by those of every regiment that has been for a time stationed on the Rock, as well as by the observations of the civil practitioners. Dr Grey, physician to the Mediterranean Fleet, and for some years to the Naval Hospital in this garrison, after having stated, in a report to the Transport Board, that the bilious remittent fever is more or less endemic in Gibraltar during the summer and autumnal months, and pointed out with great accuracy such local causes as may generate the disease, thus describes the following symptoms as attending its aggravated state.

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* "Marsh and swamps," says Dr Blackburne, in his Remarks on Infection, page 92, "are far from being the only sources of miasmata, hereafter perhaps more justly named febrilizing gases. The foul shores of the sea—the moist slime and mud on the banks of great rivers, and of mill-ponds—the mire and mud in the unpaved streets, ditches, lanes, and passages of great towns and cities, villages, &c. particularly the cellars and damp abodes where the poorest classes are most frequently doomed to dwell—the moats of garrisons, &c. &c.—the soil where certain hospitals, barracks, or encampments are situated—the wells and cellars, damp cells, and dungeons of prisons, and the holds of ships, are all calculated to emit pyrexial effluvia from the moist earth, mud, or filth, which are most commonly to be found within their precincts."

† Marshy exhalations by themselves will complicate, but are not sufficient to produce the yellow fever, to the developement of which, according to an author who for many years had opportunities of observing the complaint, must concur a certain degree of heat, the insalubrity of the place, the idiosyncrasy of the subject, and an occult atmospheric disposition.

" Great prostration of strength; excruciating headach, referring to the forehead and orbits; eyes turgid; flushed countenance; oppression at the præcordia; general pains of the back, loins, and calves of the legs; great irritability of the stomach, and bilious vomitings; yellow suffusion of the skin, and, towards the fatal termination of the disease, hemorrhages from the nose and mouth, with black fœtid stools. I have also observed in some instances," Dr Gray adds, " the matter ejected from the stomach of a dark colour, resembling coffee-grounds. During the long period of serving I cannot recal to my recollection a single instance of fevers being communicated from one person to another, either amongst medical attendants or nurses."—(London Medical Repository for November 1817, page 417.)

Staff-Surgeon Glasse, whose veracity is so well known in this garrison, where he had resided ten years, says in a document which has been published, " During the autumn, I have been in the habit of seeing solitary cases of fever attended with black vomiting and other severe symptoms, both in the town and south, without the disease being communicated to others confined in the same building."* It remains for me to add, that malaria produced by exhalations from common sewers, probably from its being more especially the result of animal decomposition, appears to exert a most powerful influence over the character of this fever; and of the truth of this fact, which I have had frequent occasion to observe while I had the medical charge of the Dock-Yard Department, and of the poor in the south district of the garrison, I shall mention only two striking instances.

In 1813, the first patient that I saw with the black vomit lived in a gully at the south end of the South Pavilion, in the close vicinity of a drain, the noxiousness of which had been repeatedly reported to the authorities; and I have been under the necessity of sending to the Civil Hospital, at different periods, from the same neighbourhood, three gardeners, two of whom rapidly died of fever in that establishment.

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* Out of regard for truth, I must observe, that since 1816, cases of this description have been far less numerous in this garrison, which I have no hesitation to ascribe to the improved state of cleanliness.

From 1819 to 1822, I repeatedly had under treatment at the Naval Artificers' quarters at Rosia, in the latter part of the summer, cases of fever attended by irritability of the stomach, yellowness of the skin, and other severe symptoms, some of which terminated fatally; and having observed that a common sewer in the centre of that building was then in a defective state, and occasionally emitted very offensive effluvia, I reported the circumstances to the Commissioner of the Royal Navy in this garrison, as mentioned in my annual report of 1823; the drain was removed, and from that period I have not seen or heard of any similar attack of fever occurring in the establishment.

During the late epidemic, the first cases of fever in the south appear to have occurred in the neighbourhood of the gully, near the bridge leading to the hospital, where the offensive vapours of a large drain was very perceptible during the summer months; and the disease has been especially severe on that spot, as in every other place deprived of a pure air and free ventilation.

Query 21st.—Were critical days observed in the course of this fever?

Answer.—I believe in its violent attacks the progress of the disease has been so rapid that no critical days have been observed; but, in milder ones, the crisis has varied from the third to the fifth day, and in those cases where the disease has assumed a remittent form, the crisis has not taken place until the seventh, ninth, and sometimes until the fourteenth day.

Query 22d.—Report at large on the treatment, but especially on bleeding, purgatives, cold affusion, tepid ditto, mercury as an alterative, or mercurial oily remedies, sulphate of quinine, &c. &c.

Answer.—General bleeding, which had been found useful in the epidemic of 1814, was resorted to from the first breaking out of the disease, both in large and small quantities, but with sanguine temperaments; and under the most marked indications I experienced no favourable result.

I may add, that having tried it occasionally at subsequent periods, it never produced more beneficial effects. Topical bleeding by leeches has materially contributed to relieve the headach, and to check the incipient gastric symptoms.

I have constantly made use of purgative medicines at the commencement of the attack, and I have repeated them occasionally in different stages of the disease, so as to keep the bowels open. Oily purgatives have generally been preferred, as being milder in their effects, and less liable to increase the irritability of the stomach.

Cold affusion has been very useful in moderating the preternatural heat of the skin, and checking the increased velocity of the circulation.

Tepid affusion, or fomentations, have been frequently applied on the abdomen or epigastric region, when there was any pain, strangury, or diminution of urine; and the warm-bath has been found highly useful in the second stage of the disease, when the pains were severe, and the nervous symptoms attended by great prostration of strength.

Mercury has been most frequently employed in the treatment of this disease; sometimes it has been given in large doses at the commencement of the attack, but more generally has been administered as an alterative, in small and repeated doses; and when resorted to in the first instance, it has undoubtedly been the most valuable remedy in the treatment of this formidable disease, producing beneficial effects not only by its purgative quality, but likewise by its specific action on the hepatic system, and probably by affecting, through the medium of the circulation, the secretory surfaces, then in a state of high irritability and inflammation. Its effects, however, have not always been in proportion to the quantity introduced, but have been chiefly indicated by its influence on the salivary glands, producing a degree of ptyalism which had been followed by a quick and rapid change in all the symptoms, and has afforded the surest prognostic of a favourable termination.

I have nevertheless to lament that, in some of the violent attacks, the system, from causes which I shall not pretend to explain, has appeared unsusceptible of the action of that powerful remedy, even administered in large doses, and with a scrupulous regularity; and the event has then been invariably fatal.

When the increased action of the system had been overcome, and when the remissions were no longer doubtful, which generally happened about the sixth or seventh day, the sulphate of quinine was eminently useful in strengthening the debilitated absorbents, and restoring the energy of the digestive organs.

Query 23d.—What were the sequelæ of this disease, visceral obstructions, ague, &c.?

Answer.—The rapid progress, and the short duration of this disease, leave no time for visceral obstructions to be formed; and the only sequelæ which I observed were, excessive weakness, the impaired state of the digestive organs, and, as a necessary consequence, a protracted and tedious convalescence.

Query 24th.—Were other acute diseases observable during the epidemic, particularly intermittent fever?

Answer.—No acute diseases have been observed during the epidemic. Intermittent fever is generally a disease foreign to this Rock.

Query 25th.—Were persons who had suffered once, or had had the yellow fever of the West Indies, liable to a second attack? state the number of each.

Answer.—I believe a first attack of the epidemic in Gibraltar generally produces an immunity from a second attack; and the experience acquired during the different visitations of this disease, has inspired the inhabitants with a well-founded confidence on this subject. There are, however, some exceptions; and I may specify two well-marked cases, which had been witnessed by me in 1814, and which have been stated to the Board established in this garrison to investigate that question. I have heard of a few more from incontestable authorities. With respect to the yellow-fever of the West Indies, I have not met among my patients with any that stated himself to have suffered from it in that part of the globe.

Query 26th.—What the state of health in the adjacent parts of Spain and Barbary?

Answer.—I have not heard of any particular alteration in the general state of health in the adjacent parts of Spain and Barbary, with the exception of a few suspicious cases of fever, which occurred in Algeziras during the latter part of October and commencement of November. Those cases which occurred in a yard where there was a number of lodgers, most of them of the lower class, appear to have been marked with symptoms similar to those in this garrison: seven individuals died within a very few days in the same premises; and the disease did not spread, although the intercourse with the sick was not prevented, and although several of the neighbours deserted the place to remove to different parts of the town, which facts furnish a strong argument in favour of the opinion, that the disease had been produced by causes peculiar to that spot, and that it was not in Algeziras, any more than in Gibraltar, propagated by contagion.

Such are the answers which my observations and experience enable me to give to the proposed queries. During the long period of my services in this garrison, I have had the melancholy opportunity of witnessing repeated visitations of the disease, and the circumstance of the epidemic having been constantly limited to the atmosphere of the Rock, beyond which the patients, whether they recovered or died after removal, have never communicated the disease to their families or attendants, has convinced me that the Gibraltar fever differs from those disorders transmissible by the contact of persons or things to whatever place the virus may be carried; and that it is a disease of an endemial character, produced by local causes, combined with accidental circumstances, and so completely dependent upon those causes, that, when their influence ceases, it can no longer be propagated, as it does not contain a reproductive power in itself.

This character of the disease, founded on the evidence of facts, leads to the inference, that cordons of troops, or any sanitary measures which tend to keep the population within an infected district, will only aggravate the evil, and be attended with that fear and consternation so fatal in every epidemic; and that the progress of the disease may only be effectually checked by restoring, if possible, the healthfulness of the locality, and at any rate, by a speedy removal from the source of sickliness.

Gibraltar, 24th February 1829.

Art. III.—*An Inquiry how far the operation of Trachedtomy may be considered advisable in those instances in which a foreign body is lodged in either Bronchus, with observations on some late cases of that accident.* By John Browne, M. D., Member of the Royal College of Surgeons in Ireland, and of the Dublin Chirurgical Society; one of the Surgeons to the Hospital of St Andrew and St Anne, Dublin. [Read at the Dublin Chirurgical Society, December 9, 1829.]

THE attention of the surgical profession was very strongly excited some months ago by the novel and bold attempt of Mr Key to extract a sixpence from one of the bronchial tubes; and although the operation failed, from causes over which the operator had no control, its subsequent completion after the death of the individual fully demonstrated its practicability.

Before I had read the account of Mr Key's operation, the untimely fate of Dr Gilroy's patient, [*] with whom I was personally acquainted, induced me to consider whether she might not have been saved by a similar proceeding, the result of which inquiry I shall now detail.

It was proved several years ago by Desault, [†] that although the *glottis* and its *rima* were particularly irritable, so much so that the slightest touch of any extraneous substance, however small, was sufficient to put the whole of their muscular apparatus into spasmodic action, and excite the most violent and convulsive cough; yet the lower part of the respiratory canal possessed a degree of sensibility very little above that of other mucous membranes, and that it would admit, for any indefinite time, of the passage of elastic tubes and other chirurgical agents; and this fact, which has been also noticed by modern authors,[‡] was soon after applied to practical purposes by the introduction of such tubes into the trachea, in cases of spasmodic affections of this canal, of laryngitis, and of severe wounds. [*]

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[*] Edinburgh Medical and Surgical Journal, Vol. xxxv. p. 293.

[†] Œuvres Chirurgicales par Bichat, Tome ii. p. 242, 3d edition.

[‡] Porter on the Pathology of the Larynx, p. 186.—Beclard in Dictionnaire de Médecine, Tome iii. p. 513.—Liston in Medical Gazette, Vol. iv. p. 381.

It was also long ago ascertained by experiments on animals, [†] that foreign bodies driven down into one or other *bronchus*, as far as the structure and dimensions of these tubes and the utmost force of the operator would allow, were almost immediately afterwards ejected violently in expiration, through a wound made in front of the trachea. [‡] Analogy anticipated a similar result in the human subject, founded on a knowledge of the elasticity and muscularity of the air passages, [§] and on acquaintance with several cases in which the foreign substances, when not angular or irregular, had moved up and down in the trachea, and had been driven out with violence immediately on an opening being made in that tube. [||]

We thus had, on the one hand, a proof of the power of the air-passages to expel such bodies as were sufficiently smooth to pass freely up and down in these tubes through an artificial opening in the common canal; and, on the other hand, a strong presumption that, were such foreign bodies sharp, angular, and impacted in the *bronchi*, there was no reason to expect any serious impediment to the introduction of such instruments as were calculated to extract them. Yet, with such information, it is strange, that, until the present year, no operation of the kind had been ever attempted.

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[*] "A l'hôpital de Lyon un soldat, que le chagrin avait porté à se détruire, a été nourri et a respiré pendant quinze jours, à l'aide de deux sondes introduites l'une dans le larynx, l'autre dans l'œsophage."—Œuv. Chir. de Desault, Tome ii. p. 251.

[†] See M. Favier's Experiments in Mémoires de l'Académie Royale de Chirurgie, Tom. v. p. 536.

[‡] "Des corps de toute espèce et de toute figure introduits dans la trachée-artère de chiens vivans en ont été chassés avec force, quoiqu'ils eussent été poussés fort avant du côté des bronches."—(Mém. de l'Acad. Tome v. p. 537.)

[§] "L'elasticité joue le plus grande rôle parmi les propriétés du canal aérien; il la doit particulièrement au tissu fibreux qui entre dans sa composition. C'est en vertu de cette élasticité que l'air sort de la poitrine dans l'expiration ordinaire; l'action des fibres musculaires fixées aux cerceaux cartilagineux peut également y contribuer."—(Beclard in Dict. de Médecine, Tome iii. p. 575.) "As the mucous membrane is prolonged into the vesicles forming their lining, it is a legitimate matter of inference, from the various facts disclosed by the general anatomy of tissues, that the fibrous lamellæ and muscular structure extend to them also."—(Quain's Elements of Anatomy, p. 573.)

[||] Pelletan relates two cases of beans lodged in the tracheæ of children, which were thrown out to a great distance on an opening being made in that tube.—(Clinique Chirurgicale, Tom. i. p. 5.) And Boyer, giving an account of a similar accident, tells us, that "the foreign body did not present at the wound made in the operation, nor did he make any attempt to extract it, as he relied on the powers of nature." His opinion was proved to be correct, for in "three hours afterwards the bean was expelled from the wound in a violent fit of coughing and sneezing, which had been excited by giving the child a pinch of snuff."—(Traité Des Maladies Chirurgicales, Tome vii. p. 142. 4ter.) Mr A. Burns, in narrating the case of a young woman in whose trachea a plumb-stone had been lodged for two days, but without producing the usual irritation, says, "I examined her carefully, and ascertained, that while she was taking air into the lungs, the foreign substance descended with rapidity along the trachea to the point where it bifurcates, from which, during expiration, it was again forced up into the larynx."—(Surgical Anatomy of the Head and Neck, p. 418, 2d edition.)

But although authors have not directly advised the trachea to be opened, or the *bronchi* to be searched, when foreign bodies were lodged in these last named passages, they have occasionally hinted at the utility of such practice.

In an interesting case detailed by Lescure,[*] of an engraver who had let a *louis-d'or* slip into the trachea more than five years before, and who died phthisical, the coin was found placed perpendicularly in the upper part of the right lung, at the bifurcation of the *bronchus* on this side. In this instance, the foreign body had been originally fixed behind the cricoid cartilage, " where," says the author, " it remained many years, and where," continues he, " *the action of the air would probably again have pushed it, had an opening been made shortly before his death into the respiratory tube*."

M. Sue, in relating the case of a girl who let a portion of the back bone of a pigeon slip into the trachea, and who died of consequent consumption seventeen years after, though she expectorated the bone eighteen months previous to her decease,[†] tells us that she first complained of pain in the upper part of the trachea, under the larynx, where it remained for seven years; and that it then shifted to the upper part of the chest. " During the first seven years," says he, " bronchotomy might have been done with success, and *perhaps would have given exit to the foreign body, even if practised at a very late period of her illness*;"[‡] and in confirmation of this last supposition, he notices the experiments of M. Favier. M. Sabatier,[§] speaking on this subject, allows " that if, after opening the tube, the extraneous substances neither present themselves at the orifice, nor are driven out of it, we should not hesitate to *search for them* with forceps or any other instrument capable of procuring their extraction." Mr Allan Burns, alluding to M. Favier's experiment, says, " this experiment, uniform in its result, proves that bronchotomy will be equally useful when the extraneous substance has *descended into the trachea*, as when it has been impacted in the larynx."[||] M. Marjolin, talking of the mobility of such substances in the trachea, observes, " that they must sometimes, however, be seized with forceps, or extracted with a blunt hook."[*] The author of the article " Bronchotomy," in Rees's Cyclopædia, says, " an assistant then draws asunder the sides of the wounded trachea with a blunt hook, upon which the operator introduces a crooked forceps, with which he *carefully seeks for*, and extracts, the foreign body."[†] Pelletan also tells us,[‡] that " in such cases art must be employed." M. Jourdan, describing the operation, says, " the foreign body escapes at the very moment, often with noise, for *the air shut up in the lungs is sufficient for its expulsion, without our having occasion to seize it*."[§]

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[*] Mémoires de l'Académie de Chirurgie, Tome v. p. 528.

[†] Ibid. Tome v. p. 533.

[‡] " Et peut-être même l'incision de la trachée-artère ne seroit elle pas contre-indiquée dans cette dernière circonstance; car, apres avoir ouvert une voie à la sortie du corps etranger, il seroit possible de déterminer une toux salutaire qui en feroit l'expulsion."

[§] De La Médecine Opératoire, Tome iii. p. 29, 2d Edition.

[||] Surgical Anatomy of the Head and Neck, 2d Edition, p. 418.

Some authors, with similar intentions, have advised the wound in the trachea to be kept open, and the expulsion of the foreign body to be solicited by fumigations, &c. if it can neither be seen or felt in the wound upon ordinary searching.[||]

M. Louis was not an equally warm advocate for such measures,[¶] and Porter, referring to M. Sue's case, says, " when an accident of this kind occurs, it scarcely comes within the reach of surgical relief, and its progress must be left to the operations of nature, which, in some instances, are sufficient to work a cure. At all events, bronchotomy would promise but little, unless we could be certain that the offending substance was so placed as to be easily laid hold on and extracted."[**] M. Dupuytren, from the adoption of similar ideas, appears to have neglected bronchotomy in a case where, probably, it would have succeeded.[††] I hope to be able to show, in the course of these observations, that, in many cases, smooth or rounded foreign bodies move to the trachea from one or other *bronchus*, and, if an opening be made in the former tube, are expelled therefrom; and that in other instances, when sharp and unequal, they remain fixed from the beginning; but that, even in such cases, they may be laid hold of by forceps, and extracted.

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[*] Dictionnaire de Médecine, Tome iii. p. 537.

[†] Volume v.

[‡] Clinique Chirurgicale, Tome i. p. 6.

[§] Dictionnaire des Sciences Médicales, Tome iii. p. 314.

[||] " Si le corps étranger ne peut être aperçu ou senti, il ne faut pas fatiguer le malade par des recherches multipliées et toujours douleureuses. On se borne alors à couvrir la plaie avec un morceau de gaze : on a aussi conseillé, dans ce cas, de tenir ses bords écartés avec des lames de plomb arrondies et repliées, et de solliciter de temps en temps la toux par des fumigations légèrement irritantes."—(Marjolin in Dict. de Médec. T. iii. p. 531.)—" M. Dupuytren, dans un cas semblable n'ayant pu découvrir une féve de haricot qui se trouvait dans la trachée-artère d'un enfant, pansa le malade, et le lendemain on trouva ce corps dans l'appareil."—Sabatier de la Méd. Opér. Tome iii. p. 88. Sanson's Edition.

[¶] He says, talking of bronchotomy, " cette opération ne seroit plus indiquée, si le corps etranger avait pénétré jusques dans la substance du poumon."—Mem. de l'Acad. de Chir. Tome iv. p. 529.

[**] Pathology of the Larynx, p. 202.

[††] The case was that of a young man who let a fifty centime piece slip into the trachea. " Aux premiers accidens succédèrent des accès irréguliers de toux et de suffocation, durant lesquels le corps étranger semblait *parcourir toute l'étendue de la trachée-artere*. Pendant les intervalles de calme rien n'annonçait la situation de la pièce, et le sujet éprouvait à peine de la gêne a respirer. *Il désirait toutefois ardemment l'opération ; mais il aurait été peu rationnel de la pratiquer, alors que le corps étranger était fixé, et que l'on ne pouvait connâitre son siége. On attendant*

The foreign substances most usually taken into the air-passages are nuts, kernels of fruits, peas, beans, comfits, various kinds of soft aliment, grains of corn, bullets, button moulds, divers coins, rings, spiculæ of bone, fish bones, pins, and needles. *

These bodies, particularly when large, will generally pass eventually into the right *bronchus*, giving rise, in their course there, to the various symptoms produced by a greater or less degree of obstruction to the admission of the air to the lungs. In the trachea, when smooth and round, they are generally moveable, ascending in expiration, and descending in inspiration, and often excite pain in all parts of this tube in succession. Should they be spiculated and uneven, they commonly soon become fixed, the patient feeling increased pain in that situation, † and also pointing to it with his finger. The use of the stethoscope, combined with careful examination of the circumstances of the accident, will likewise materially assist in forming a judgment of the case. ‡

The impaction of foreign bodies in the *bronchi* is of less frequent occurrence in children than in adults; and it is fortunate that it is so, as the narrowness of the glottis, the smaller size of the tracheæ, and the greater sensibility of the organs in general, in young persons, render such accidents more severe.

I should fear that in these instances it would be very difficult, if not impossible, to introduce the suitable extracting instrument into the air-passages; but there could be no objection to making an opening in the trachea. §

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que ce corps devînt mobile de nouveau et que les accidens se reproduisissent, le malade mourût d'une pneumonie aigue."—Sabatier de la Méd. Opér. T. iii. p. 84, Sanson's Edition.—In this instance the coin appears to have penetrated eventually into one of the *bronchi*; and although there was no local pain to indicate its presence, it is likely that it would have been propelled through the opening had tracheotomy been performed.

* Dict. de Méd. T. vi. p. 56.—Dict. des Sciences Méd. T. vii. p. 12.

† "Dans d'autres cas le corps étranger est adhérent à un point de la trachée-artère, ou bien il se trouve arrété transversalement; alors la douleur est fixe."—(Dict. des Sciences Médicales, Tome vii. p. 14.)

‡ "L'examen soigneux des circonstances commémoratives, l'exploration du pharynx et de l'œsophage avec une sonde flexible, l'analyse soigneuse des symptômes présentés par les malades, pourront, dans presque tous les cas, procurer des notions certaines sur ces diverses circonstances."—(Marjolin, in Dict. de Méd. Tome vi. p. 56.)

§ I remember to have seen a child of three or four years of age, in whom a piece

In a late case, in which a piece of a perforated plumb-stone was lodged in the trachea, and where tracheotomy had been performed unsuccessfully, the use of the stethoscope having twelve days before demonstrated that the foreign body was lodged in the right *bronchus*, the original wound was dilated downwards, and the stone, (which had previously again moved up to the trachea,) was, almost instantaneously, expelled in a fit of coughing fourteen days after the accident. This case shows very strongly that foreign bodies move to the trachea from the *bronchus*, if an opening be made in the former tube, and are expelled.

In Mr Key's case, during the first operation, on passing a long probe upwards, "great irritation and coughing were excited, but very much less when it was passed downwards;" a fact confirming Desault's observation as to the comparative irritability of the mucous membranes of the larynx and trachea. Previous to the second operation, experiments on the dead subject, and information derived from percussion and the use of the stethoscope, enabled the operator to discover, not only that the foreign body was in the right *bronchus*, but also its exact situation there; and how valuable the information obtained by the use of the stethoscope may be in such cases, was still farther proved shortly before death; for "while breathing with great difficulty, the left side of the chest alone seemed to be distended, the right being almost motionless;" a fact clearly showing the obstruction to the entrance of air into the right *bronchus* by the foreign body; and which could not have arisen from the diseased state of the corresponding lung, as, on dissection, about the upper third of *both* lungs appeared to have suffered from repeated attacks of inflammation.

A similar occurrence has been noticed in other cases where foreign bodies were suspected to be lodged in the *bronchi*;* no respiratory murmur, or at least a very feeble degree of it, being audible, on one or other side, in such intances.

Mr Key, very judiciously, previous to his operation, examined the calibre of each *bronchus*, and ascertained that the left *bronchus* would merely allow "a sixpence to enter, whereas the right passed in a more straight direction, and was sufficiently large to admit it to the distance of an inch." The right *bronchus*, therefore, being more a continuation of the trachea

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of delft had been forced into the right *bronchus*. There was a fixed pain in the upper part of the thorax, on the right side, rather below the level of the first bone of the sternum. The parents would not allow any operation, and the child died on the third day.

* London Medical Gazette, Vol. v. p. 75.

than the left, and also being larger, and a little more forward, (Cloquet,) is that in which we should expect, most generally, to find foreign bodies lodged; but, from the inaccurate manner in which most cases of this description have been recorded, we are able to verify this supposition in a small proportion only.

The observation of Mr Key must be understood only in contradistinction to the greater obliquity of the left *bronchus*; for all modern anatomists, such as Meckel, Beclard, Cloquet, Quain, &c. agree in stating that the right *bronchus* passes nearly in a transverse direction, in its course to the right lung. Through the kindness of Mr Alcock, (of the Medico-Chirurgical School, Park Street,) I was enabled to repeat Mr Key's experiments on the dead subject, and with nearly the same result. I found, however, 1*st*, That a shilling could be forced, for the distance of an inch, into the right *bronchus*, but not at all into the left; while a sixpence passed freely into either *bronchus*, but farther within the right; 2*dly*, Also, that if a long slender forceps was passed into the trachea, without giving it any particular direction, it went invariably into the left *bronchus*; and, *lastly*, That bones, sixpences, &c. could be extracted with equal facility from either of these tubes; as, by inclining the handle of the forceps towards the left side, the transverse direction of the right *bronchus* was, in a great measure, counteracted. A sixpence would not enter the trachea of a child of eight years. Anatomists state the length of the right *bronchus* to be one inch, and its width two-thirds of an inch; and the length of the left *bronchus* to be two inches, and its width half an inch.—(Meckel, Beclard, &c.)

In Dr Gilroy's case, the offending substance lay "in the upper part of the right *bronchus*, close to the bifurcation of the trachea." In Lescure's case, before adverted to, it was lodged "at the bifurcation of the *bronchi*, upon the right side;" and in the cases reported by Messrs Bell,* Key, and M'Namara, and in one narrated by Marjolin † in the same situation. And in a remarkable case which lately occurred in this city, in which the greater part of a molar tooth had passed through the *rima glottidis*, at the moment of its extraction, in consequence of a sudden inspiration, "the tooth lay in the right bronchial tube, not far from the tracheal division, and was easily removed by forceps." In this instance it was thought that the tooth had been swallowed; a supposition which was apparently confirmed by the very slight nature of the symptoms for some days; but at that period severe pneumonic symptoms set in, which terminated fatally in the third week from the accident. On dissection, pleuripneumony and bronchitis were found in an intense degree. I am indebted for these particulars to Mr Houston.

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* Lond. Med. Gaz. Vol. v. p. 75.

† Dict. de Med. T. iii. p. 591.

Extraneous substances, notwithstanding, occasionally enter the left *bronchus*. M. Lenglet, surgeon at Sedan, tells us of a soldier, who, whilst sucking a marrow bone, was suddenly attacked with a severe cough, accompanied by pain in the left side of the chest, and spitting of blood, followed by a copious expectoration of very fœtid ash-coloured pus.

After many months of suffering, he threw up, during a long and terrible fit of coughing, a sharp and triangular piece of bone. He no longer felt pain in his chest, and all the symptoms were much relieved, yet he died, nevertheless, on the third day. On dissection, the right lung appeared sound, but the left was in a state of suppuration; four inches below the bifurcation of the trachea, on the left side, was an unnatural cavity large enough to hold a nutmeg, and here the foreign body had remained ten months. The situation of the pain had denoted its position from the very first. [*]

It may be said, that, even allowing this operation of searching the bronchial tubes to be necessary in some cases, still in a great majority it will not be required; as, in most instances, nature will rid herself of the offending substance by suppuration and expectoration. Of such occurrences we have, it is true, many examples. But unfortunately, before this natural process is accomplished, so much injury has been done to the respiratory apparatus, that a large proportion of the cases die consumptive, even after the rejection of the foreign body. This was the case in the instances related by Lescure, Sue, and Lenglet, and we find many others recorded. Thus Pelletan [†] relates the case of a boy twelve years old, who swallowed a flint; a violent and constant cough, with pneumonic symptoms, succeeded. Bleeding, blisters, emetics, &c. were used for twenty-two days without relief. An opening was made into the trachea,—the flint being felt without; but the patient feeling it moving back and forwards in the passage, Pelletan thought it was kept back by its weight, [‡] he therefore laid the boy flat on his side, and immediately it was driven out with noise. Here the cough continued, the expectoration became purulent, and death ensued, from phthisis, eight months after the accident. In another fatal case of consumption in a male adult, reported by the same author, a large piece of woollen stuff or shred, was expectorated a short time before death, and without influencing in any degree the progress of the disease. Desault [*] tells us of a man breakfasting on cherries, who, in quarrelling with a friend, let a stone slip into the larynx. This produced convulsive cough, dyspnœa, pain, &c. Desault advised an operation, but was overruled by others; and the patient died two years afterwards of laryngæal phthisis, the cherry-stone being found on dissection to have given rise to it. A case nearly similar [†] is narrated of the senior monk of the Abbey of Saint Martin, near Treves, who, attempting to swallow a cherry suddenly, let it slip into the trachea. A violent cough, threatening death, succeeded, but after some hours went off. In a year afterwards, he was attacked with cough and fever, and threw up a mass as large as a nutmeg, formed of calcareous matter, with the cherry-stone for a nucleus. Copious purulent expectoration followed, and he died in a few days.

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[*] Mem. de l'Acad. de Chir. T. v. p. 539.

[†] Clinique Chirurgicale, Tome i. p. 10.

[‡] "Quelquefois ils restent dans les bronches ou à la partie inférieure de la trachée; mais ce cas est rare; il faut pour cela que ce soit une balle de plomb ou tout autre corps très pesant."—*Breschet in Dict. Des Sciences Méd.* Tome vii. p. 14.

Among the recoveries are the following. A case related by Tulpius, [‡] of a citizen of Amsterdam who had been harassed for more than seven years by an obstinate cough, and a difficulty of breathing, which had reduced him to the last extremity. He threw up at last, whilst coughing violently, a piece of a nutshell as large as the finger nail. A case mentioned by Hochsteter, [§] of a resident of Augsburg, who, with the idea of curing himself of the toothach, had put into his mouth a golden Portugal ducat, and imagined he had swallowed it in his sleep. But some time after his voice becoming very hoarse, with emaciation, it was suspected to be in the windpipe; and, in two years and a half he ejected it by the mouth. Bartholin [‖] tells us of a girl at Padua, who, having laughed while swallowing a nut, was immediately attacked with a violent cough, which tormented her exceedingly, for two months, bringing on fever and marasmus; but she spit it up at length, and was cured. Louis [¶] notices a case from Stalpart Van der Wiel, of a girl who let a bit of a veal bone slip into the trachea, in the act of swallowing broth. A constant cough, fever, hæmoptysis, and ulceration of the lung, ensued; but, at the end of four months, she coughed up the bone, and thus escaped consumption.

Bonetus [**] relates an example of a cherry-stone passing into the trachea, producing immediate cough and sense of suffocation; but in three weeks the man coughed it up and recovered. Claubry [††] mentions a case of consumption lasting fourteen years, and accompanied, for the last four, by hæmoptysis, cured by the expectoration of a piece of bone. Mr Holmar describes a similar case which had gone on for fifteen years, accompanied by cough, bloody sputæ, and hectic diarrhœa, and which was also speedily relieved, in consequence of the bony fragment, three quarters of an inch long, and apparently carious, being suddenly coughed up, after the discharge of a pint of blood.[*] Dr Arnot informs us of a case of consumption, from which the patient recovered on coughing up a bone which he had swallowed two months before.[†] And Borelli[‡] tells us of a gentleman who became consumptive from a piece of nutmeg slipping into the larynx, and who, by coughing it up, recovered from a state of extreme danger.

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[*] Œuvres Chirurgicales par Bichat, Tome. ii. p. 258, 3ieme edition.

[†] Ephemerides Curios. Nat. Decad. ii. Ann. x. Obs. 66, p. 723.

[‡] Lib. 2. Observ. 7.

[§] Observ. Decad. 6, Cap. x.

[‖] Hist. Anat. Cent. 2. Hist. 27.

[¶] Mem. de l'Acad de Chir, Tome iv, P. 536.

[**] Med. Septen. Collect. Lib. 2. Sect. 9, de Affect. Asp. Art. Cop. 8.

[††] Sedill. Journ. Gen. 34, page 13. 1809.

These last cases are sufficiently numerous, and might be multiplied, proving, what indeed every one must acknowledge, that in this and most other cases of disease or accident, much must be left to the judgment and discretion of the medical attendant. It appears, however, that sharp rugged substances, as bones, &c. are more liable to produce violent symptoms than smooth bodies, as cherry-stones, &c.; and that recovery more frequently follows the expectoration of the latter than of the former kinds of extraneous matters.

Louis did not place much reliance on the efforts of nature in such instances;[§] and other practitioners have coincided in his opinion.[||] The case of Mrs N—— (before referred to) strongly demonstrates the uncertainty of the resources of nature. Here was a strong constitution, likely to bear up against pain, suppuration, and hectic symptoms, until the spontaneous discharge of the offending substance; and yet we find death ensuing within a shorter period than in most of the other similar instances. The impression on the mind of her medical attendant, likewise, that her complaint proceeded from exposure to cold, is worthy of notice, as it is a mistake which has been made in other instances, and is a dangerous one. Thus, in the case of Bonetus,[*] "the surgeon who visited him not having perceived the stone, the suffocation having ceased, and the cough alone remaining, it was considered that the first symptoms had been the effect of a catarrhal humour to which the man was liable; he having exposed himself to rain in the morning." In Lescure's case,[†] "the breathing was so difficult, that he was presumed to be asthmatic; and with this idea had been made to use many remedies." And Lenglet's case[‡] was treated as pleuritis.

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[*] Lond. Med. Journ. Vol. vii. p. 120.

[†] Edinb. Medic. Essays. Vol. ii. page 613.

[‡] P. Borelli, Hist. Med. Phys. Cent. Quest. Paris, 1656.

[§] He says, after having written a great deal on the subject, " quoiqu'il en soit de cette éjection d'un corps étranger de la trachée-artère, peut on, en cas pareil, se fier aux ressources incertaines de la nature, après les lumières que nous a fournies l'observation des cas malheureux." (Mêmoires de l'Academie de Chirurgie, Tome iv. page 531.)

[||] Thus Nacquart, talking of such possible recoveries, says, " Enfin l'expectoration ayant entrâiné le fragment, la santé s'est rétablie en quelques cas; mais le plus souvent l'altération des organes étant portée trop loin, les accidens ont continué et déterminé la mort." (Dict. des Sciences, Méd. Tome viii. p. 304.) And Marjolin, discussing the same question, thus expresses himself. " On ne peut pas compter sur ces chances favorables." (Dict. de Med. T. vi. p. 58.) Boyer likewise says, " On a vué quelquefois, à la vérité, ce corps rejeté dans un violent accès de toux; mais les faits de ce genre sont si rares, et les examples contraires si nombreux, qu'il n'est pas permis de compter sur cette ressource incertaine de la nature." (Traité Des Maladies Chirurgicales, Tome vii. page 114.)

The bone in Mrs N———'s case lay "in the right bronchus, close to the bifurcation of the trachea," and being fixed in the same position from its impaction in the mucous membrane, and somewhat higher than the sixpence in Mr Key's case, I have no doubt that its extraction might have been accomplished with the same or even greater facility; it is to be regretted, therefore, that such an attempt had not been made a day or two after the accident took place, and previous to the occurrence of inflammation and ulceration.

It is impossible to insist too strongly on the danger of delaying the operation in these cases as well as in those of foreign bodies lodged in the larynx and trachea, since every moment increases the tendency to congestion in the vessels of the head and lungs; from which causes alone many have perished after the most successful opening of the trachea, as has been proved by the concurrent testimonies of many authors.[§]

From what has been said, the following inferences appear to be established.

1. That the existence of foreign bodies in one or other *bronchus* can be ascertained by the use of the stethoscope; by the seat of the pain and other uneasy sensations; and by the previous history of the case.

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[*] Med. Septen. Collect. Lib. 2. Sect. 9, de Affect. Asp. Art. Cap. 2.

[†] Mémoires de l'Académie de Chirurgie, Tome v. page 528.

[‡] Ibid. page 532.

[§] Marjolin, giving an account of tracheotomy, says, "Mais pour qu'elle réussisse, il ne faut pas attendre que les poumons se soient engorgés, qu'il se soit formé une congestion dans le cerveau, que les malades soient expirans." (Dict. de Médecine, Tome. iii. p. 524.) And Jourdan observes, "Si l'accident dure depuis quelques jours, l'air, refoulé vers le poumon, brise les cellules de cet organe, s'insinue dans son tissue propre, et produit un emphysème qui peut devenir mortel lorsqu' étant fort étendue, il ne permet plus à l'air extérieur de pénétrer dans les cellules bronchiques comprimées." (Dictionnaire des Sciences Medicales, Tome. iii. p. 3.2) Desault says, "Le poumon engorgé par le sang, qui n'a pu y circuler perd son action organique, ne peut expulser l'air qui y est encore renfermé, se débarrasser des fluides qui l'engouent, se dilater pour en recevoir de nouveaux, et le malade périt malgré la communication retablie avec l'air extérieur." (Œuv. Chirurg. par Bichat, Tome. ii. p. 264.) Pelletan, in a case where the operation had been too long delayed, found, on dissection, "excessive fulness of the cerebral vessels." (Clin. Chir. T. i. p. 3.) And other practitioners, as Louis, Bell, Porter, &c. give similar statements.

2. That since the effect of such bodies in these unnatural situations, is to excite inflammation and abscess, most commonly ending, sooner or later, in death, it is incumbent on us to attempt their extraction with the least possible delay.

3. That small round bodies move freely from the *bronchi* to the trachea, particularly when an opening has been made in the latter, and that the best mode of promoting their expulsion is by such an operation.

4. That when sharp and angular substances have descended into either *bronchus*, they generally become fixed there, but may be extracted by forceps or other suitable instrument passed through an artificial opening in the trachea.

5. That the sooner such an operation is undertaken, the greater will be the chances of success, as the presence of the extraneous substance must give rise to congestion and inflammation in the lungs, and to various cerebral affections, all depending on mechanical interruption to the natural course of the circulation.

6. That although occasional recoveries have ensued subsequent to the spontaneous ejection of foreign bodies from the *bronchi*, such cases are rare; and the greater number of persons so circumstanced have died at longer or shorter intervals.

ART. IV.—*A Case of Pulmonary Abscess, caused by the lodgement of a Chicken Bone in one of the Bronchi.* By PETER GILROY, M. D., Member of the Royal Physical Society of Edinburgh; Resident Physician at Navan, in the county of Meath; and Physician to the Fever Hospital of that town.

MRS N———, a widow lady, forty years of age, of a robust habit, and previously remarkable for strength of constitution, was seized, while eating her dinner, on the 8th of August 1826, with a sudden and violent fit of coughing, threatening suffocation. On recovering, she told some friends who dined with her, and who were greatly alarmed for her safety, "that a chicken bone had gone wrong, and still was sticking in her chest." By this time, however, she breathed freely, and her alarm gradually went off.

The next day she felt her chest oppressed, and complained of a slight tickling cough, with inward soreness at the top of the sternum, and general uneasiness.

She sent for an intelligent apothecary, who, conceiving her illness to have arisen from exposure to cold, took some blood from the arm, and directed aperient medicine, by which treatment she was so much relieved, as to be able to go, in a day or two afterwards, some miles into the country; but the cough and other disagreeable sensations continued, though in a less degree than before. These symptoms had increased at the end of a fortnight, but were again mitigated by a second venesection, and by a repetition of the aperients.

On the 13th of September, about five weeks after the accident, I saw her for the first time, in consequence of a farther increase of the symptoms. I found her in bed, with her shoulders particularly low; her countenance was anxious, with great despondency; pulse 96, full; tongue loaded and yellow; some appetite. She attributed her illness to the same cause as before, and referred the seat of pain to the top of the sternum, towards the right side, where she felt confident the bone still remained.

I was struck with the manner in which she lay in bed, and inquired the cause. She told me "that, as long as she remained perfectly quiet, with her shoulders depressed, she was free from cough; but as soon as she raised herself in the least, or turned on either side, a violent fit of coughing came on, which she could excite when she pleased, by placing herself in the first mentioned position."

The truth of her assertion was soon verified, as she had occasion to elevate the body considerably; and the fit, which immediately ensued, was more violent and more convulsive, if I may use the expression, than any paroxysm of spasmodic asthma I had ever seen. On such occasions she usually experienced a difficulty of expectoration, as if from some mechanical obstacle, and an intolerable fetor from the throat was perceptible, not only to herself, but also to those about her.

From the above facts, I had no doubt that the bone had fallen into the trachea; but, as suppuration had taken place in the lungs, accompanied by hectic fever, little could be expected from the resources of art.

She lingered until the 29th of October, and then died, exhausted by pain, irritation, and discharge. From the period of my first visit, she could scarcely move in any direction without the occurrence of a most violent cough, apparently about to end, every moment, in suffocation.

On examining the thorax, twenty-four hours after her decease, a large abscess was found in the centre of the right lung, the greater part of which was occupied by it. The cavity of the abscess contained about twenty ounces of pus, of a reddish brown colour, and very fetid odour. The piece of chicken bone (very light and porous, and weighing only six grains,) lay in the superior part of the right *bronchus*, close to the bifurcation of the trachea; this tube here communicated with the upper part of the abscess.

Few cases of this sort have been recorded by British practitioners; but I have no doubt that if inspections were more frequently made, many such would be discovered.

Dr Mason Good,[*] talking of the causes of phthisis, says "The exciting or occasional causes are very numerous, as mechanical irritation of the lungs from swallowing a piece of bone." —"A lodgement of some fragment of a bone even in the œsophagus has in like manner been a frequent cause of phthisis, which has often been protracted through a long period of time."

Dr Young[†] mentions some examples of consumption, in which a piece of bone and other cutaneous substances were coughed up, and the patients recovered.

The older French surgeons have been more industrious, and have detailed several analogous cases, as may be seen in the writings of Louis,[‡] Pelletan,[§] &c. Among the moderns, Broussais[||] gives the following curious example. "A soldier, aged thirty-three years, strong and muscular, was wounded by a musket-shot at the battle of Novi, in 1799. The wound passed obliquely from the right side of the neck, and injured the œsophagus, but healed up after the usual time. From this period he was liable to cough, but for five years had no other complaint. He then became affected with dyspnœa and night shivering, preferring to lie on the left side, and complained slightly of his chest. His pulse was a little accelerated, and his breath fœtid; and he died on the sixth day. On dissection, the left lung was hepatized throughout, and generally adherent. Seven or eight abscesses were found in its substance, some as large as a hen's egg, others smaller. Near the root of the lung, and close to the larger bronchial tubes, lay the ball lodged in a cyst, and surrounded by a portion of the lung in a hard and callous state. Fabricius Hildanus[¶] gives a still more remarkable case of a man wounded by a small sword in the chest. The wound healed, but emaciation and phthisical symptoms remained; and, after being reduced to great extremity, he threw up, at the end of three months, whilst coughing, two tents, which (while it was yet the practice to cram wounds with foreign substances,) had been allowed to pass through carelessness into the substance of the lung. Tulpius* mentions a similar case in a Danish nobleman, where a tent was coughed up at the end of six months.

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[*] The Study of Medicine, Vol. iii. p. 260, 261, second edition.

[†] On Consumptive Diseases, p. 227.

[‡] Mém. de l'Acad. de Chirurg. Tome iv. p. 548.

[§] Clinique Chirurgicale, Tome i. p. 10.

[||] Histoire des Phlegmasies Chroniques, Tome ii. Obs. 53, p. 105.

[¶] Opera Omnia, 1682.—Centuria Prima, Observ. 46, p. 41.

One of the most remarkable instances on record is related by M. Sue,† and is that of a girl between eight and nine years of age, who, whilst eating a pigeon, let a piece of the back-bone slip into the trachea. She experienced at the moment acute pain under the larynx, with violent cough, which calmed itself by degrees; but a hissing rattle remained, especially when she wished to speak. The pain continued in the same place, and when violent was relieved by bleeding only. Such was her state for seven years. She was now sixteen, when the pain changed to the upper part of the thorax, the rattle and hissing noise disappearing at the same time; and she was free from dyspnœa for four months. At this period she was attacked suddenly with a violent cough and spitting of blood, which, although relieved by repeated bleedings, became periodical, recurring every two or three months for five years, accompanied by emaciation, &c. These symptoms, however, at length all ceased,—she appeared quite restored, and married at the age of twenty-one. For three years afterwards she was well, being only annoyed occasionally by slight pain in the chest, and bloody expectoration. But in the fourth year the hectic symptoms re-appeared, with purulent expectoration, cough, and emaciation; and after two years of farther suffering, during a violent accession of cough, she threw up the bone, which had remained seventeen years either in the windpipe or lung. She felt considerably relieved in consequence; but the symptoms of hectic continued, and she died in a state of marasmus in eighteen months afterwards.

Leighsbrook near Navan, September 1, 1830.

Art. V.—*Cases and Observations in Medical Jurisprudence.* [Continued from Vol. xxxiii. p. 76.]—*VIII. Trial for Poisoning with Sulphuric Acid.—IX. Two Trials involving the question of burning before or after Death.* By Robert Christison, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c.

Since the publication of my last medico-legal reports twelve months ago, several extremely interesting cases have occurred to me both in the civil and criminal courts of this part of the kingdom. From these I select for the present paper a very important case of murder by poisoning with sulphuric acid; and I have added, by favour of my colleague Dr Duncan, the leading circumstances of two singular trials for murder, which occurred some years ago, and which involved the question,—whether death preceded the burning of the body, or was occasioned by burning?

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* Lib. 2, Observ. xv.

† Mémoires de l'Académie Royale de Chirurgie, Tome v. p. 533.

The first case will, I have no doubt, be perused with interest by every reader, not only on account of the striking nature of the whole circumstantial evidence, the tracing of which reflects the highest credit on the local authorities,—but likewise on account of the pointed and satisfactory evidence of the medical witnesses of Aberdeen, whose investigations, in a case of much complexity, have very seldom within my experience been equalled in accuracy and conclusiveness. The case is also important as illustrating the principle I have several times endeavoured to inculcate,—that it is sometimes possible to infer poisoning absolutely from symptoms and morbid appearances alone. Another circumstance too of some moment is, that the chemical part of the case has led me to reconsider the process at present employed for the detection of sulphuric acid in the contents of the stomach and other organic mixtures,—to satisfy myself of its insufficiency and fallaciousness,—and to devise a new method of analysis, which is annexed to the present narrative, and which I trust will be found less objectionable.

The chief subject of inquiry in the second part of this communication is one of no small consequence. The question, whether a body has been burnt after death, or the individual was burnt to death, occurs not unfrequently on trials. I have remarked several cases of the kind in the English papers during the last eight years; there is no doubt that the question must occasionally occur at coroner's inquests and Sheriff's precognitions; and the foreign records of medico-legal practice show that it is not uncommon on the continent. Yet it is singular that, so far as I am aware, no researches have hitherto been made with the view of deciding the criterions by means of which the question may be answered. On this account I have made some experiments on the subject; and the results, which will be enumerated at the close of this paper, appear satisfactory, and such as will furnish in every instance a simple and infallible set of tests. For the introductory account of the two criminal cases, one of which happened at Glasgow, and the other at Leith, I am indebted to the kindness of Dr Duncan, then Professor of Medical Jurisprudence, who was a witness in the second of the two trials, and contributed materially by his evidence to the acquittal of the prisoner.

CASE VIII.—*Trial for Poisoning with Sulphuric Acid—Opinion deducible from Symptoms and Morbid Appearances only—New Processes for detecting sulphuric acid in stains on clothes, and in the contents of the stomach.*

Mrs Humphrey, a butcher's wife in Aberdeen, was tried there at last Autumn Circuit for the murder of her husband by administering to him sulphuric acid; and she was charged with perpetrating the crime by pouring the poison down his throat while he was asleep.

In most cases of murder by poison, it is extremely difficult to separate the medical and general evidence from one another. In the present instance the difficulty is peculiarly great; and for this reason, as well as on account of the singular interest and conclusiveness of the whole chain of circumstantial proof, I shall relate the general evidence at some length.

The prisoner and her husband had long lived on indifferent terms with each other. They both frequently got drunk, and quarrelled; in their quarrels blows as well as foul language were exchanged; and the prisoner on several of these occasions expressed to various witnesses a desire "that some body would give her husband poison, so as she were kept clear of it."

On Friday the 16th April her husband was seen by many people, his maid-servant included, in a state of perfect health. During the whole of that day he was at his stall in the market, and had his meals there, as well as various drams of whisky, in company with several acquaintances, none of whom suffered in consequence, any more than he himself appeared to do. Returning home at eight in the evening, still apparently in perfect health, he held a drinking party with some male friends in his kitchen; while the prisoner had her female friends to drink with her in a room adjoining. When they had both got somewhat drunk, they quarrelled and interchanged blows, on the subject of one of the prisoner's acquaintances, whose character was not to Humphrey's liking. After this, and not long past eleven, the company retired; the house was cleared for the night of all strangers; and the street-door was bolted inside by the servant.

There were now in the house only the prisoner, the deceased, and their female servant. It is also necessary to add, for the thorough understanding of the narrative, that the house consisted below stairs of a kitchen, a room entering from it, and a lobby, and above stairs, of the servant's room immediately over the kitchen, and another apartment usually kept locked, and used by the prisoner as a store-room.

Immediately after the visitors withdrew, the deceased went to bed in the kitchen, and the prisoner, who commonly slept up stairs with the servant after a quarrel with her husband, retired to the room on the ground-floor adjoining the kitchen, and at the same time desired the servant, who was usually last out of bed, to go to sleep and leave her,—saying that she wanted to see whether her husband would "settle," and adding, "Lord God, that any body would give him poison and keep her hands clear of it." The servant then extinguished the light in the kitchen, at which time she conversed with the deceased in bed, who was perfectly well to all appearance, and not particularly drunk. Proceeding now up stairs, she undressed, and had not been long in bed when she became aware that the deceased had fallen asleep; for he was quite quiet; whereas, when he lay awake after a fit of drunkenness, she invariably heard him through the floor talking aloud to himself. In a little the prisoner arrived up stairs on her stocking-soles, to see if the servant was asleep; and on the latter requesting her to come to bed, she still declined and returned down stairs, repeating that "she wanted to see her husband settle first." The servant then fell asleep. She had not slept more than a few minutes, however, when her mistress came again up stairs, still on her stocking-soles, and awoke her,—desiring her, with a smile, to go down and see what was the matter with her master. She did so: And then, within half an hour after leaving Humphrey quite well, she found him crying out with pain, writhing from side to side, and exclaiming that he was all burnt in the inside. After some conversation as to the cause of his illness, she went out to seek assistance, and in doing so found the bolt of the street-door undrawn, as she had left it.

This witness, as well as a former servant, further stated, that her master, when he was drunk, always slept on his back with his mouth wide open. She likewise deposed that her mistress when she awoke her, although intoxicated, was quite sensible enough to understand what she was doing,—that she carried her light steadily, walked without staggering, and spoke distinctly.

The conduct of the prisoner after her husband's illness commenced was not such as to lead to any material evidence against her. On the contrary, after she began to recover from her state of intoxication, she showed great apparent concern for his sufferings, attended his sick-bed unremittingly, cried often, and threw no obstacles in the way of medical advice being immediately procured. To some neighbours, however, whom she called to his aid, she said he was mad; and throughout his whole illness, without any imputation being thrown on her, she made frequent attempts to get him to say before witnesses that he did not ascribe his sufferings to her.

The deceased never directly charged the prisoner with having occasioned his misfortune; but he constantly insisted that, whatever it was he had got to do him harm, he got it in his own house; that he went to bed quite well and awoke with his throat and inside roasting; and that the stuff which did him harm, must have been poured into his throat while he was asleep. He never said a single word to justify the slightest suspicion of suicide or accident; which besides were farther disproved by the servant, who, both on extinguishing his light, and, on going down to him at the prisoner's request, did not observe any bottle near him or other vessel which could have contained a liquid. When questioned as to the person whom he suspected to have been the author of his misfortune, he repeatedly said he did not know, but at the same time gave broad hints that he suspected his wife, repulsed at first her proffers of kindness, and refused to "clear her," when she requested him to do so. On one occasion, however, shortly before his death, he did make use of expressions which might have been interpreted as exculpating her. This was the only article of general evidence in the prisoner's favour; but it was not in my opinion of any importance, because, as the expression used was a simple negative in reply to the question whether "he had any reflexions to make against his wife," and was not spoken till after their reconciliation, it might be interpreted as implying, that he had forgiven her.

In most trials for poisoning the proof of the administration of the poison by the prisoner is very defective. But in the present instance it was established by a complete chain of minute, but very remarkable circumstances, partly medical, partly general.

The subsequent medical evidence will show satisfactorily that poison was administered after the deceased went to bed, and that the poison must have been sulphuric acid. It is farther plain from the preceding part of the evidence, that the poison must have been administered by some person in the house, that suicide or accident was out of the question, and, therefore, that the crime was perpetrated either by the servant or the prisoner. The same evidence also attaches strong suspicion to the prisoner; but her guilt was clearly established by the following particulars.

Some weeks before this time, a quantity of sulphuric acid, amounting to about three teaspoonfuls, was purchased for the prisoner, who applied it by means of a wire to burn away a wart. The phial in which it was kept stood at the kitchen window, opposite the bed and at some distance from it. At five in the afternoon of Friday the servant saw the phial in its usual place containing about two teaspoonfuls; but at night, after the alarm occasioned by Humphrey's illness, one of the neighbours found the bottle in the same spot with only a few drops in it.

Now the prisoner's friends who had been drinking with her in the low room in the early part of the evening left two wine-glasses there, which were also seen by the servant when she left the prisoner in that room, before going up stairs to bed. But after the alarm, four or five men and women, who went into the same room, found also a third wine-glass. One of these women had with her an infant, who was constantly stretching out his hand to the glasses; and she gave him one of them and put it to his lips. The child instantly screamed violently, which drew her attention to the glass; and on remarking that it was moist, she put her tongue to it, and immediately felt "as if a lancet had been thrust into the tongue." The other people having tasted it after her and experienced the same effect, the prisoner was called in and told of what had happened. She replied that it must have been the dregs of an alum gargle she was using for a sore throat, and took the glass away with her to the kitchen. She was there seen by the servant to wipe it with an apron; and some days afterwards the apron was found with red and corroded spots on it. After removing the glass the prisoner returned with some alum gargle in a tea-cup to the party who had called her attention to the glass; and these people all tasted the alum gargle, but declared it did not taste the least like the contents of the glass.

The medical history of the case was to the following purport. From the first moment the deceased was seen by the servant about midnight, he was complaining of burning in his throat and inside, and said he awoke suddenly with these symptoms. He had likewise extreme difficulty in articulating, and frequent retching, froth issued from his mouth, he tried to swallow water but could not, and on attempting to swallow milk he returned it curdled. About one, *Mr Jamieson*, who was called to see him, remarked the symptoms now mentioned, and also observed, that the lips were uninjured, the tongue and inside of the mouth very white, the whole throat dark and inflamed, the uvula black, the countenance pale and ghastly, and the pulse very feeble. He tried to draw blood from the arm, but got only a few ounces. Having afterwards procured the assistance of *Dr Murray*, an attempt was made to use the stomach-pump, as the man was quite unable to swallow; but the tube of the pump could not be passed, on account of the pain in the throat. The suspicious of the medical gentlemen as to the administration of poison were at this time somewhat lulled. The man and his wife having misled them in describing the time and manner of the commencement of the symptoms, they began to think his complaint might be a malignant sore throat. They therefore administered first a salt injection, then another with laudanum, and directed warm fomentations to be applied to the stomach.

At half-past seven the same morning, Humphrey still complained of his stomach, the tongue was swollen, the breathing difficult, and about this period was also remarked for the first time a reddish mark on the chin and another at the angle of the mouth, both of which marks in the course of two or three hours became brownish. A sinapism was now applied to the throat, and an ounce of castor oil administered slowly and with difficulty.—At eleven in the forenoon he was in the same state and unable to swallow any thing.—At eight in the evening his throat and stomach were rather easier, and he could swallow with much difficulty a little liquid. His medical attendants were now convinced that some corrosive acid had been administered; and accordingly magnesia was given.

On the forenoon of Sunday there was much difficulty in breathing, which the man ascribed to his sore throat. Leeches were therefore applied, and with some relief to the breathing.—At eight in the evening the pulse was extremely feeble, and the man's strength so much exhausted, that it was obvious he could not live many hours longer; and accordingly he expired about eleven the same evening,—forty-seven hours after the commencement of his illness.

The body was examined thirteen hours after death by *Drs Henderson*, and *Murray* and *Mr Jamieson*, who drew up the following report of the appearances.

" We certify, &c. &c.

" Externally the body appeared muscular and fat. The scrotum and penis were slightly swollen, and they were livid, more especially the glans. A small quantity of whitish liquid was noticed between the prepuce and glans. On the outside of the mouth there were two brownish marks, in length between one and two inches. One of these was on the centre of the chin, and its upper end, which was nearly a quarter of an inch from the edge of the lower lip, was narrower than the part below. The other was of a lighter colour, and situated near the left corner of the mouth. The gums and part of the inside of the lips were of an almost milky whiteness; but neither the remainder of the lips nor the teeth exhibited any thing worthy of notice. The roof of the mouth had a glazed appearance, and was of a greyish-white colour except at the posterior part, where a degree of redness was visible. The uvula was ash-coloured. The back part of the tongue had lost its investing membrane and was of a red colour, while its fore-part was covered with a whitish brown crust. In appearance, the pharynx was similar to the posterior part of the roof of the mouth. The membrane covering the epiglottis was all around ash-coloured and much thickened, detached in some places, and in the rest easily separable from the parts beneath, which had a florid appearance. On the inside of the larynx, a little below the rima glottidis, a small portion of the membrane was white and in part detached. An in considerable quantity of purulent matter was detected in the muscular substance between the larynx and os hyoides. The gullet we were disposed to consider narrower than usual throughout its whole course, and on the inside it was dry and completely divested of its mucous coat, except that several narrow, yellowish stripes of this membrane remained.

" On the posterior surface of the outside of the stomach we observed a reddened portion with numerous small blood-vessels branching through it. This organ contained an inconsiderable quantity of gas, along with about three ounces of a thick, reddish liquid. The greater part of the inside of the stomach presented an unequal surface, being overspread with numerous erosions or ulcerations of no inconsiderable depth, and in various irregular forms, though mostly in that of branched or winding furrows, whose surface had in colour a resemblance to blood. Near the cardia there was a rather extensive ash-coloured crust; and a space still larger on the posterior part, corresponding with the red portion of the outside above described, exhibiting red dotting and arborescence. Near the pylorus a yellow crust was noticed, and in the part of the duodenum next the stomach there was a crust similar to that just-mentioned, but in greater quantity, more flaky and of a brighter colour. The lower part of the duodenum was distinctly red on the inner surface. The remaining intestines as far the rectum were examined inside and outside; but with exception of a red patch on the jejunum, and another on the left arch of the colon, there was nothing that requires to be mentioned.

" The contents of the intestines were a thick, brown, uniform liquid, in small quantity; and about three ounces of bloody serum were met with in the cavity of the abdomen. Those parts of the abdominal viscera, which have not been already noticed, were found entirely natural, as well as the whole contents of the chest and head. It may be added that the stomach and part of the duodenum, the contents of the stomach with its washings, and the contents of the intestines with their washings, were preserved for future examination.

" From the appearances we are led to the conclusion, that

James Humphrey had swallowed a highly irritating or corrosive substance, which was the cause of his death." *Signed*, &c. Aberdeen, April 23, 1830.

It will presently be seen from the chemical report, that no sulphuric acid could be discovered in the contents of the stomach or intestines. As to the phial which contained the dregs of the acid supposed to have been administered, it was broken to pieces by one of the neighbours as soon as it was discovered.

The only chemical evidence in the case was derived from the examination of the dress and bed-clothes of the patient. The shirt, sheet, blanket and bed-cover, worn by him on Friday night, presented various stains and corroded spots; but his coat, trowsers, and other articles of dress, which lay on a chair close to the bed, were uninjured. The stained articles were found by two sheriff-officers who visited the house on the Saturday evening, and who left instructions that they should remain untouched. The prisoner, however, removed them; and they were subsequently found locked up in her store-room above stairs. On the Thursday and Saturday after the man's death, portions of the injured as well as uninjured parts of each article of dress, were committed to Messrs Henderson, Murray, and Jamieson, for analysis; and on the 4th of June, exactly seven weeks after the man's illness commenced, fragments of the injured part of the blanket and bed-cover were sent to Edinburgh for my examination.

The report of the proceedings of the Aberdeen gentlemen was as follows.

" We certify, &c. * * * *

" On the 22d, 23d, 26th, 27th, 28th, and 30th, we proceeded with the chemical examination of the stomach, the contents of the bottles and the clothes.

" And we have to report as the result of this examination, that we did not find any poisonous matter in the stomach or in the contents of any of the bottles; but that we ascertained the presence of uncombined acid in certain portions of one of the bed-covers, of the blanket, of the sheet, and of the shirt; and that we found this acid was sulphuric by the application of compounds of barytes, which gave precipitates that were insoluble in nitric or muriatic acid, and which when heated along with charcoal, and afterwards placed in contact with water and muriatic acid, gave sulphuretted-hydrogen gas; whose presence was indicated by its blackening a piece of paper dipped in a solution of a salt of lead, and also in most of the experiments by its smell.

" We performed at the same time comparative experiments on other parts of each of the above-named articles; from which we did not obtain any appreciable quantity of sulphuric acid." *Signed*, &c. 30th April 1830.

The articles sent to Edinburgh were a fragment of a bed-cover with one broad stain, a fragment of a blanket with two small stains, and portions of precipitates procured by the Aberdeen gentlemen with the barytic test from the solution of the stains they examined. These articles I subjected to analysis; and the following report was returned.

"I certify that on the 26th ult. I received from the Crown agent a sealed packet containing ten small parcels, described as precipitates procured by Drs Henderson, Murray, and Jamieson, in the course of experiments made by them to discover sulphuric acid in certain stained or injured spots of a shirt, a sheet, a blanket, and two bed-covers. Five of the precipitates, described as having been procured from the stained or injured parts of these articles, consisted of small quantities of white powder; in each of which I satisfied myself of the presence of a large proportion of sulphuric acid by the same method of analysis which the Aberdeen gentlemen employed. The five remaining precipitates, which were described as having been procured from uninjured portions of the same shirt, sheet, blanket, and bed-covers, by the process applied to the injured portions, were so minute in quantity that I did not think it necessary to subject them to farther examination.

"From the mode in which the various precipitates were obtained, and my own examination of them, I am of opinion that sulphuric acid existed in considerable proportion, and in an uncombined state in the stained portions of the shirt and bed-clothes.

"I likewise certify, that on the 4th instant I received from the Crown agent another sealed packet, containing a fragment of a blanket and of a bed-cover, with labels attached and signed, &c.

"On the blanket there were two pale brown spots hardly so big as a sixpence, where the texture of the material was somewhat rotten, and its fibres swelled and glued together. These spots were subjected to the action of successive portions of boiling distilled water; and the same operation was performed with a fragment of equal size from an uninjured part of the blanket.

"There was thus procured from the uninjured part a solution which was not acid, and which by a process analogous to that employed by the Aberdeen gentlemen, was found to contain a mere trace of sulphuric acid in a state of combination. But the stained fragments yielded a solution which was strongly acid, and which by the same process was found to contain a quantity of free sulphuric acid, large in proportion to the size of the stains.

"These experiments satisfy me that the unstained part of the blanket did not contain any uncombined sulphuric acid, and that the stained spots contained, in relation to their size, a large quantity of this acid in an uncombined state. The quantity was so great, that in my opinion the spots were occasioned by an acid nearly of equal strength with the ordinary sulphuric acid of the shops.

"On the fragment of the bed-cover there was one yellowish-brown stain of the size of a half-crown, where the texture of the cloth was rotten. This spot, and a portion of the bed-cover where it was unstained, were subjected to the same process as the blanket, and with the same results, except that the quantity of sulphuric acid in the stains was considerably less."—Signed, &c.—June 1830.

In the commentary which will be presently added to the case I shall supply some farther facts relative to the analysis. Meanwhile let me conclude the narrative by detailing the principal points brought under the consideration of the medical witnesses at the trial, with their opinions on them. These points related chiefly—to the cause of death as deduced from the whole medical facts,—to the possibility of inferring death by poison without reference to the state of the bed-clothes,—and to the possibility of the poison being administered during sleep, or taken before the deceased fell asleep. The medical examination, exclusive of the reports and the account of the man's illness, which have been already detailed, was to the following effect.

Mr Jamieson, surgeon in Aberdeen.—Was called to see the deceased, &c. Formed the opinion from the history of the case and the appearances, that he had swallowed some highly corrosive substance, which was the cause of death. There were no appearances to account for death in any other way: The body was very healthy. The strongest whisky could not produce the effects observed. Found no marks of acid on any part of Humphrey's clothes except his shirt. From the appearances in the dead body, the history of the man's illness, and the analysis of the marks on the clothes, has no doubt sulphuric acid was the cause of death. Two or three teaspoonfuls of acid, if concentrated, would cause the effects observed. If deceased had taken the acid while awake he would not have fallen asleep after: sulphuric acid, when tasted, produces immediate pain. Could not form any opinion from the marks near the mouth, whether the deceased had been lying on his back when he got the acid. The acid might be poured into his throat while he was asleep, if he lay on his back with the mouth wide open; and in this way all the appearances observed might have been produced.

Dr Henderson, physician, and assistant lecturer on chemistry in the University of Aberdeen.—Saw Humphrey on Sunday, &c. From the appearances during life and in the dead body, came to the conclusion, that some highly corrosive substance had been taken. Two or three teaspoonfuls of acid would produce the appearances. Considered from the course of the stains at the corner of the mouth that the man must have been on his back when he got the acid. The mark on the chin was made apparently by a stronger acid than that on the corner of the mouth. A drop falling on the chin would produce the mark there.—The acid usually sold in the shops is very strong, highly concentrated. There were no appearances in the body to account for death, except the marks of inflammation in the throat and stomach.

Cross-examined. Is not positive as to the quantity of acid which might be necessary to produce the appearances observed. Cannot tell from the analysis the precise degree of concentration of the acid which occasioned the stains; but it must have been considerably concentrated to produce the corrosions.

By the Court. If a few drops of acid in a glass, tasted with the finger, occasioned a sense of burning, this acid must have been pretty much concentrated. From the medical evidence heard in Court, and his own investigations, he has no doubt death arose from sulphuric acid.

Cross-examination resumed. Some of the acid must have reached the stomach. The inflammation there could not have arisen from the inflammation in the throat spreading along a continuous membrane. The statement made by a witness, that, on wiping the deceased's mouth with an apron on Sunday, the apron was burnt, is hardly reconcilable with his afterwards finding no poison remaining in the stomach: under the circumstances scarcely thinks sulphuric acid could have produced that corrosion.

Dr Murray, physician in Aberdeen.—Visited Humphrey, &c. From the whole particulars of the case is satisfied some highly corrosive substance was taken; and sulphuric acid might have had the effects observed. Excluding the analysis of the clothes, cannot say positively that sulphuric acid was the corrosive substance. But thinks it most probable this was the poison.

Dr Christison.—Identifies Report and various parcels. Dropped on an entire portion of the fragment of blanket sent from Aberdeen two drops of concentrated sulphuric acid, and examined these spots in seven weeks, which was the interval between the production of the spots mentioned in the report, and his examination of them. Found the appearances of corrosion exactly the same, and the quantity of sulphuric acid remaining nearly the same. Concludes, that the corroded spots sent from Aberdeen were made with an acid nearly as strong as that usually sold by apothecaries, (see p. 310.) It was almost impossible Humphrey could have taken sulphuric acid before falling asleep. He could not have fallen asleep after it, the pain produced being immediate and violent. He could not have taken it while awake, without knowing, unless he had been much more intoxicated than appears from the evidence to have been the case.—Excluding the analysis of the marks on the clothes, considers it clear, from the history of the man's illness and the appearances in the dead body, that some corrosive poison was the cause of death. Sulphuric acid would produce all the effects, and it is the only strong acid which could produce the *brown* mark on the chin. Nitric acid, the only other common acid which could produce the rest of the appearances would have produced a bright yellow spot. The man must have awoke immediately, if strong sulphuric acid was poured into his mouth,—so that he might spit some of it on the bed-clothes. He might not, however, awake so broadly as to discover who poured it in.

Cross-examined. Cannot account for the corrosion of the handkerchief said to have been produced on the Sunday by wiping Humphrey's mouth. The witness who stated this circumstance must have been mistaken,—unless more acid was administered recently before.

Such were the leading particulars of the medical and general evidence brought out at the trial. With such a mass of circumstantial proof against the prisoner, and scarcely a single circumstance to throw doubt over any part of it, a conviction was inevitable. She was subsequently executed in pursuance of her sentence, and before her death made a full confession of her guilt.

Observations.

The remarks I have to make on the case will be directed to two subjects,—1. The evidence of poisoning, as deduced from symptoms and morbid appearances only; and 2. Some particulars relative to the chemical analysis, together with a few observations on the general process for detecting sulphuric acid in organic mixtures.

1.

Since my attention was turned to the medical jurisprudence of poisons, I have on several occasions endeavoured to combat a notion which prevails among most continental medical jurists, as well as among practitioners in this country, whether conversant or not with toxicology, that it is impossible, without chemical evidence, to form more than a presumptive opinion in favour of poisoning. In several papers, as well as more lately in my Treatise on Poisons, I think it has been successfully shown, that this doctrine, sound undoubtedly as a general rule, admits nevertheless of frequent exceptions; and on two judicial occasions, where the prisoners were condemned, I considered it necessary to act according to the principles advocated in my publications, and to deliver an opinion in favour of the highest probability, if not certainty, of poisoning from symptoms and morbid appearances only.

The present case is in some respects a strong confirmation of the opinion I have maintained. The discovery of sulphuric acid in the shirt and bed-clothes was undoubtedly a very strong article of chemical evidence. But if the proof of poisoning with a corrosive poison, as founded on other circumstances, had been materially incomplete, this article of evidence would not have been so important as it proved to be; since it would then have been far from impossible that the clothes were injured on some other occasion than during the administration of poison. The proof of poisoning, however, was completely established from the history of the illness and the morbid appearances, without the aid of chemical evidence at all. The man went to bed quite well; awoke suddenly with an excruciating sense of burning in the throat and stomach; from that very moment was very nearly unable to swallow, and spoke with difficulty in a whisper; presented a white glazed tongue and gums, and in the course of ten hours a brownish mark on the chin, such as sulphuric acid is known to produce when it does not actually excoriate; and died under the preceding symptoms within forty-eight hours. What other cause, except the administration of some powerful corrosive, could produce this exact train of symptoms,—so suddenly arising,—and ending fatally so soon? But farther, in the dead body the gums and fore-part of the tongue were found white, the throat and back of the tongue dark or red, with spots like eschars, almost the whole gullet divested of its mucous membrane, and the stomach also presenting unequivocal signs of violent irritation. What natural disease could produce these appearances conjunctly in so short a time as two days? What other poison could produce them conjunctly so soon except some violent corrosive? In a similar case no witness can err in stating, as the medical gentlemen of Aberdeen and myself did, that there could be no doubt death arose from some violent corrosive; and I still think, that the occurrence of the brown spot, which when recent has a very peculiar appearance, left scarcely any doubt that the corrosive substance (poisoning of some kind being proved) was sulphuric acid.

These observations are the more important, that in cases of poisoning with sulphuric acid evidence can on the one hand be seldom expected from the chemical analysis, on account of the fluidity and easy miscibility of the poison with all other liquids, —while, on the other hand, evidence from symptoms and morbid appearances may almost always be procured of the most conclusive kind.

It may be well to add here, that the peculiar white glazed appearance of the tongue and gums,—an important particular of the evidence from symptoms and appearances,—is most characteristic during the first and commencement of the second day. In a case of recovery I have seen it disappear before the beginning of the third day, a thick crust having been thrown off the tongue, and the subjacent part of that organ left red and raw-looking. On the gums the appearance remains longer, and the whiteness and glazing are sometimes such as to give the gums a resemblance to ivory.

2.

The process of analysis employed by the Aberdeen gentlemen was the same with that described in my Treatise on Poisons, and likewise in my account of the trial of Euphemia Macmillan in the 31st volume of this Journal, p. 234. In my Treatise I have said that this process could easily detect two-thirds of a grain of sulphuric acid in a fragment of cloth. But in the present instance its delicacy proved to be much greater. The sulphate of baryta procured from the injured spot of the bed-cover weighed when heated to redness not quite a fifth of a grain; and that procured from the spots on the blanket weighed a grain and a third; which quantities indicated about a fourteenth of a grain, and half a grain of the common sulphuric acid of the shops.

In my evidence it is stated, that an express experiment was made with two drops of the concentrated sulphuric acid of the shops on an entire piece of the blanket, in order to determine what proportion of the acid might remain after seven weeks;—the object of the experiment being to discover whether the acid found on the clothes in Humphrey's house had been concentrated when dropped on them. In the two corroded spots sent from Aberdeen, I procured 1.35 grain of sulphate of baryta seven weeks after they were made. From two spots purposely made on the same blanket with concentrated acid and examined likewise in seven weeks, 1.82 grain of sulphate of baryta was procured. Hence the acid in the former was to that in the latter in the proportion of three to four nearly, and must therefore have been considerably concentrated. In my experiment exactly one-half of the acid employed had disappeared. The sulphate of baryta indicated 72-100ths of a grain of the strong hydrated acid remaining in the cloth, while the weight of the two drops at the commencement was 1.44 grain.

The process I have given for the detection of sulphuric acid in the stains of clothes is not liable to any fallacy, when taken along with the corroded state of the stains, and a comparative analysis of the uninjured parts of the clothes. Yet it may be improved, as will be seen presently. It is a circumstance worthy of special consideration, however, that no trust-worthy method of analysis at present exists in any chemical or medico-legal work, by which a small quantity of free sulphuric acid may be discovered in organic mixtures, such as the contents of the stomach. In my paper on the case of Euphemia Macmillan I have said, that the process for the examination of stained clothes is also the best for such mixtures; and, in my Treatise on Poisons, I have advised its application in both cases, with the condition, that an inference in favour of the presence of sulphuric acid is only to be drawn when the quantity of precipitate with the barytic test is large. The reason for annexing this condition is, that, although no free sulphuric acid be present, the salts of baryta may give a precipitate in consequence of the contents of the stomach containing, as they may generally be expected to do, a small quantity of neutral sulphates; while the test of acidity may likewise afford the requisite action, because there may also be present, as the late researches of Leuret and Lassaigne, Tiedemann and Gmelin, and Prout, have shown to be often the case, a small quantity of free muriatic or acetic acid, or both. In short, the whole indications of the presence of free sulphuric acid supplied by my process may be equally supplied by the co-existence of a neutral sulphate and free muriatic or acetic acid.

This is a serious fallacy which is sufficient to render the process quite inconclusive, unless the quantity of barytic precipitate is large, and it can at the same time be determined that no neutral sulphate could obtain entrance into the mixture except what naturally existed in it. The objection is stated in my Treatise, but not with the force which it merits. It has been very fully explained, however, by *M. Devergie* in an account of the case of Euphemia Macmillan translated from my paper, and published in the first volume of the Annales D' Hygiène Publique et de Médecine-Légale (Note, p. 213.) *M. Devergie* has at the same time recommended a process which he considers free from fallacy. But this talented author has fallen into the singular inadvertency of recommending a process liable to the identical objection which he very correctly charges against mine.

He advises that the experimentalist ascertain in the first place, the acidity of the fluid, and then with a few drops of it the presence of sulphuric acid, free or combined. This being done, the acidity of the fluid is to be neutralized by boiling with pure carbonate of lime, by which any free sulphuric acid is thrown down in the form of sulphate of lime, while the neutral sulphates are not altered. The precipitate is next to be washed with a small quantity of water, and then dissolved in boiling distilled water acidulated with a few drops of nitric acid; and the sulphuric acid is lastly to be thrown down by a salt of baryta.

This process, which was proposed originally by *Orfila*, is liable to two important objections. 1. In the first place, it is liable, as already hinted, to the very objection advanced by *M. Devergie* against my method. Suppose a neutral sulphate be present, along with free muriatic or acetic acid,—a concurrence by no means improbable in the contents of the stomach. It is plain that the two trial tests, directed to prove acidity and the presence of sulphuric acid in one form or another, will act as if the sulphuric acid was free. Then, on adding carbonate of lime, the free muriatic or acetic acid dissolves the lime, forming a neutral muriate or acetate of lime; between the elements of which and the elements of the neutral sulphate an immediate interchange must ensue, and sulphate of lime be thrown down. This method therefore cannot be applied, as its proposer imagines, to organic mixtures, such as the contents of the stomach. But at first view it appears well adapted to the examination of stains on clothes, where the presence of a neutral sulphate may be an inconvenience, but where the co-existence of muriatic or acetic acid is after a time impossible. Here too, however, it is liable to another important objection.—2. In the second place, then, it cannot be applied in any circumstances, if the quantity of acid is small. For in that case the sulphate of lime which is formed remains in solution, or is dissolved by the necessary washing of the mixed calcareous precipitate,—so that it cannot be detached from the fluid supposed to contain originally a neutral sulphate. For example, I have found by experiment, that a grain of sulphuric acid dissolved with a few grains of sulphate of soda in three or four drachms of water cannot be separated in the form of an undissolved sulphate of lime by powdered calcareous spar. The acid is neutralised; but the salt formed is sufficiently soluble to remain in solution with the sulphate of soda.

The principle of the process of *Orfila* and *Devergie* is, however ingenious and correct,—the object being to form an insoluble sulphate by means of a compound salt, which cannot decompose the neutral sulphates; and the process, so far as regards stains on clothes, may be made perfect by substituting a different salt for the carbonate of lime, and varying the subsequent steps of the process.

The following appears to me an extremely delicate and satisfactory process of the kind; and I am disposed to prefer it to that I have hitherto recommended and been in the practice of employing. It does away with the necessity of a comparative analysis of an uninjured portion of the clothes, and it obviates all fallacy from the possible or actual presence of neutral sulphates.

Process for detecting free sulphuric acid in stained clothes.—Having prepared a solution by boiling the stains in a little distilled water, ascertain the acidity of the liquid with litmus paper, and with a few drops determine the presence of sulphuric acid in one form or another.—Then add carbonate of lead to complete neutralization and promote its decomposition by boiling: Sulphate of lead will be formed with any free sulphuric acid present, but not with the neutral alkaline or earthy sulphates, as I have taken care to ascertain experimentally.—The carbonate of lead used in the process must of course be quite free of sulphate, which the common white-lead of the shops almost always contains; and to guard against this fallacy, its purity must be ascertained by observing whether it is entirely soluble in diluted nitric acid: or, which is still better, it may be prepared in a state of great purity by precipitating a solution of acetate of lead with *bi*-carbonate of soda,* and carefully washing the precipitate with distilled water.—The suspected fluid being neutralized by the carbonate of lead, the precipitate is to be collected on a filter, and washed once with distilled water,—then compressed between folds of blotting-paper, if possible without rupturing the filter,—then again washed with distilled water, and again dried by compression between folds of blotting-paper.—The precipitate which now consists of carbonate and sulphate of lead, coloured perhaps by organic matter, is to be treated with diluted nitric acid, which removes the carbonate. The residue, after being collected on a filter and washed with distilled water, is to be carefully mixed up, while damp, with distilled water in a mortar, and decomposed by a stream of sulphuretted-hydrogen gas, maintained for half an hour or more, according to the quantity of the precipitate. After the action of the gas the mixture is to be immediately boiled and filtered. There will then be in solution free sulphuric acid, with some organic matter. The presence of the acid may be proved now by the old process. That is, the solution is to be treated with nitrate of baryta and a few drops of nitric acid; the precipitated sulphate of baryta may be then converted into the sulphuret by heating it with charcoal; and the sulphuret is proved to exist by the disengagement of sulphuretted-hydrogen on the addition of diluted muriatic acid.

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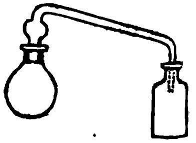
* The bicarbonate of soda of the shops is free of sulphates.

This method is superior to the plan proposed by *Orfila* and *Devergie*, because the sulphate of lead is much more insoluble than sulphate of lime, and therefore is not, like it, retained in solution when the quantity of free sulphuric acid is small. I have found it to answer extremely well with a single small drop of sulphuric acid. It is not applicable, however, any more than the French method, to the contents of the stomach and such other mixtures as may contain not only neutral sulphates, but likewise free muriatic or acetic acid. For in that case the carbonate of lead, although it cannot directly decompose the neutral sulphates, is dissolved by the free muriatic or acetic acid, a soluble salt of lead is formed, and double decomposition ensues between that salt and the neutral sulphate. These acids can scarcely be considered a source of fallacy in the instance of stains on clothes, because they will escape by evaporation after a moderate exposure to the air. If, however, there be any reason to apprehend their presence, they may be driven off before boiling the stained cloth, by simply exposing it for a short time in an evaporating basin to the heat of the vapour-bath.

In the instance of the contents of the stomach, on account of the probable presence of muriatic or acetic acid, the present process is fallacious, and a variation of it is indispensable. I have encountered considerable difficulty in fixing on a good process for the detection of sulphuric acid in such circumstances. Independently of the impediments already mentioned, namely, the combined presence of neutral sulphates and free muriatic or acetic acid, there is the important obstacle arising from the circumstance, that the quantity of sulphuric acid in the mixture must very generally be small,—because little can remain in the stomach, if, as generally happens, the patient survive some hours, and vomit much. After trying various other plans unsuccessfully, the following method has appeared to me both accurate and delicate.



Process for detecting free sulphuric acid in the contents of the stomach and other organic mixtures.— Filter the fluid, distilled water being added if necessary. Place it in a mattrass, to which a tube with a ball is adapted by a cork, as in the annexed figure; and let the extremity of the tube just enter the neck of a bottle, of the same capacity with the mattrass, and immersed up to the neck in cold water. Distil with a gentle heat, till the fluid in the mattrass is reduced to the consistence of a thin syrup. The last portions of the distilled fluid are then to be tested for muriatic acid by the nitrate of silver, and for acetic acid by the taste and smell. If neither appear to be present, the matter remaining in the mattrass may be subjected to the process for detecting free sulphuric acid in stained cloth.—If, on the contrary, either acid be present, distilled water is to be poured into the mattrass, and the mixture again distilled to the consistence of a thin syrup; and this addition of water and distillation must be repeated till *the last portions* of the distilled liquid give, with nitrate of silver, either no precipitate, or a mere haze only, or till the smell and taste of acetic acid entirely disappear. The residual fluid in the mattrass will then contain a mere trace only of muriatic or acetic acid, while scarcely any free sulphuric acid will have passed over with the water, if the distillation be properly managed. When thus prepared, therefore, the matter in the mattrass may be subjected without risk of error to the process for detecting sulphuric acid in stains.

The following points must be attended to, in order to insure the success of this process. 1. The end of the tube within the mattrass should be nearly a third of an inch in diameter, so that any fluid which condenses in the ball, may drop into the mattrass, and not fill up the whole diameter of the tube, to the risk of being propelled onward into the bottle. 2. After each distillation, the ball should be washed with a little distilled water, and the washings added to what remains in the mattrass, because some sulphuric acid generally rises as high as the ball, especially towards the close of each distillation. 3. The distillation ought never to be pushed to too great dryness, otherwise the sulphuric acid will pass freely into the bottle, or may be decomposed by the organic matter: The discharge of the muriatic or acetic acid is therefore to be accomplished rather by repeating the distillation often, than by carrying it far on any occasion. I have found that a drachm of muriatic acid, and as much acetic acid, were driven off entirely by six distillations from a solution of sulphate of soda containing two drops of sulphuric acid, without any material quantity of the sulphuric acid escaping; and in another trial with a mixture of bread, milk, muriatic and acetic acids, and four drops of sulphuric acid, the two former acids were driven off by four distillations, while almost the whole sulphuric acid remained behind.

This process I am inclined to consider a satisfactory one. I have tried it successfully on small quantities of sulphuric acid in complicated organic mixtures; but I must admit that it has not yet been in my power to make as great a variety of trials as would be desirable. An objection to it, which some may consider serious, is, that it will not indicate free sulphuric acid where any muriate or acetate is present in such quantity that the base of the salt can neutralize the whole sulphuric acid; for in the course of distillation the muriate or acetate is decomposed and the sulphuric takes the place of the muriatic or acetic acid. This objection, however, will probably apply to every method of analysis.—Another objection may be, that although this process provides for the fallacy arising from the presence of muriatic and acetic acids, it does not obviate the similar fallacy arising from the possible presence of the citric or some other vegetable acids which are not volatile. To this objection it is perhaps sufficient to reply, that none of these acids can, like the muriatic and acetic acids, form a part of the contents of the stomach as a secretion by the stomach itself; that when present they must have been directly introduced; and that this can scarcely fail to be discovered by the general evidence, so as to put the experimentalist on his guard.

CASE IX.—*Trials for murder, where the chief question was, Whether the deceased individuals were burnt to death, or were murdered before the bodies were burnt?—Experimental Inquiry into the criterions for distinguishing burns, according as they are inflicted before or after death.*

The two trials with which I shall introduce the second part of this communication happened some years ago in Scotland. In each the prisoner was accused of having murdered his wife, and burnt the body afterwards to conceal the murder. The following abstract is drawn up from the notes of Dr Duncan, and chiefly in his own words.

1. The first was the case of a man Gilchrist, who was condemned and executed. This case happened at Glasgow.

The prisoner and his wife lived on tolerably good terms, but used to take fits of rambling, and get drunk for days together. At last on one of these occasions, after their return home in the evening, the people who lived in the floor above them heard a noise like that of two persons struggling, and soon afterwards a rattling or gurgling and moaning, as of one choking or bleeding to death. They so strongly suspected all was not right, that they called down to Gilchrist through the floor, that they were afraid he was killing his wife. In no long time they were farther alarmed by the smell of fire and the filling of the house with smoke; upon which they went down to Gilchrist's apartments, and demanded admission. After some delay he admitted them, and in doing so appeared to them to have come out of an inner room, where he said he had been asleep in bed. On letting them in he stumbled over the body of his wife, who lay in the outer apartment, quite dead, kneeling before a chair, and very much burnt.

In these circumstances, the prisoner was accused of having murdered her, and then burnt the body to conceal the manner of death; while, on the contrary, he alleged he had gone to bed tired, and knew nothing of what had befallen her till he was awakened by his neighbours, and that he presumed her clothes had caught fire while she was intoxicated and burnt her to death.

"Unfortunately," observes Dr Duncan in his notes, "there were no data to decide this question. The medical gentlemen who had been appointed to examine the body merely reported that they found the body so much burnt that they could learn nothing from it as to the cause of death. The general evidence was all against the prisoner. He was accordingly condemned, although the precise manner of his wife's death was not proved even presumptively; and the sentence was put in execution; but to the very last he vehemently and solemnly denied that he was guilty."

"This case," adds Dr Duncan, "made a great impression on me at the time, and led me to reflect deeply on the deficiency of the medical evidence, and the points to which the attention of the examiners should have been chiefly turned. It appeared to me that the principal question might have been determined by examining carefully the edges of the burnt parts. It appeared to me that if the woman had been burnt to death, marks of vital reaction would have been seen around the burns; that the margin of these would have been limited and defined; and that any blisters which might have been present would have contained serum; but that if the body had not been burnt till after death, there would have been no inflammation around the burn, the effects of fire on the internal parts would have been undefined, and the blisters would have contained only air."

2. It is singular that within a year Dr Duncan had an opportunity of applying these views in a similar judicial case, which occurred at Leith.

The general evidence was of the same nature as in the case of Gilchrist, but even stronger against the prisoner. He lived on bad terms with his wife. On the evening of her death she returned home at a late hour with a lighted candle, after getting some whisky at a neighbour's. At this time the prisoner was in bed; but some time afterwards there was heard a considerable noise, like that of struggling and of chairs pushed up and down the room; and after this the man was heard in an adjoining bed-room endeavouring to quiet his child, who was crying.

Presently the neighbours were alarmed by a strong smell of fire proceeding from the prisoner's apartments. They therefore knocked at his door for admission; but in vain: All the noise they could make did not bring him to the door. At last a man forced his way in by breaking the window of the outer room. On entering, he found the room full of smoke, and observed something burning red in a corner, over which he instantly threw a pitcher of water, and which proved to be the body of the woman burning on the hearth. Several persons now entered the inner room, where they found the prisoner either asleep or feigning to be so. On being roused and told of his wife's death, he expressed neither surprise nor sorrow, but coolly demanded by what authority his neighbours broke into his house, and threatened to send for a constable to commit them. Under such circumstances, the presumption of his having been accessary to his wife's death was strong.

The dead body was examined under the Sheriff's warrant, and Dr Duncan was present. "We found," says he, "some parts of the body, especially the belly, burnt to a cinder. It was not there we could expect to find any proof whether the burning had been before or after death: the action of the fire had been too violent. We then examined the parts on which it had acted more moderately, namely, the face and extremities; and here we discovered what we were unanimous in considering to be incontestable proof that the woman had been burnt to death,—that she had been set fire to while alive, and had died in consequence of the burning. There was every mark of vital reaction; some spots merely red and inflamed; others scorched to a hard, transparent crust, but surrounded with distinct redness; and a great many blisters filled with lymph, perfectly different from those produced on the dead body, which are not filled with a fluid, but with air or vapour. In short, we found appearances exactly similar to those of fire on a living body; and therefore we reported as our unanimous opinion, that the deceased was burnt to death."

As there was no proof that the prisoner had set fire to her, he was not found guilty; but in consequence of the extremely suspicious nature of the general evidence, and especially the circumstance of the man apparently pretending to sleep in defiance of his neighbours' attempts to awake him with loud knocking,—the jury returned the intermediate verdict of *not proven*. The last fact was considered very suspicious by all present, and by Dr Duncan among the rest, who admitted that he could give no explanation of the apparent difficulty of awaking a man who had been broad awake but a few minutes before. He considered, however, that the whole apparently discordant evidence might be reconciled by supposing that the woman's clothes caught fire accidentally, and that the husband, thinking this a convenient mode of getting rid of his wife, took no steps to save her.

"However that may be," continues Dr Duncan, "I think it positively certain that in the last case, and probably also in the first, the woman was burnt to death. One circumstance worthy of particular notice occurred in both; I allude to the violent and destructive action of fire, compared with the small quantity of combustible matter consumed. In both cases these unfortunate women were burnt to death, and their bodies deeply scorched by their clothes alone; for in neither was there any trace of burning in the house or furniture. I examined the last case on the spot. The woman was found on the hearth with part of her clothes unburnt, and a chair, from which she had fallen, quite entire. She was dead when the neighbours entered, and in the dark the body was discovered by a red light issuing from it."

Observations.

This interesting narrative gives rise to various reflexions. In the first place, I think it is extremely difficult to avoid the conclusion that the body was in that singular state in which it is apt to undergo spontaneous combustion, or I should rather say, to be preternaturally combustible. It is difficult to explain otherwise the great extent of the burn which was inflicted.—Secondly, although the subsequent experiments will show that Dr Duncan was perfectly right in his opinion, and the grounds on which he rested it, an important question arises, and was indeed very ingeniously started by the Crown counsel at the trial, in reference to the opinion that the woman was burnt to death,—whether the redness and blisters remarked on the edge of the scorched parts might not have arisen, immediately after strangling or some other cause of death than burning, during the period when a lingering vitality remains in the body, and when undoubtedly certain phenomena of a vital nature are frequently observed. The medical witnesses at the trial admitted that the question could not be answered decisively, on account of the want of the necessary facts, but that they did not consider it at all probable that blisters at least could be produced even immediately after death. It will be seen presently, that they were right in regard to both blisters and redness.—Thirdly, as to the prisoner not being awakened by the noise of his neighbours, which even Dr Duncan was disposed to consider suspicious, I have to observe, that from what has come under my own observation, this circumstance cannot be considered evidence against the man. Those who have not had their attention called accidentally to the matter, will scarcely believe how profoundly some persons sleep, more especially working people after their day's labour is over. I have met with an instance where the loudest noise I could make at a door and window close to the bed where the sleeper lay did not rouse him; and a boy of my acquaintance, after a long excursion one day, not only slept out all the noise his family could make at his bed-room door, which he had locked inside, but literally continued to sleep, till his father, in a state of alarm, cut out a pannel of the door with an axe, and entering the room, shook him by the shoulders.

In an inquiry into the criterions by which a burn inflicted during life may be distinguished from one produced after death, the following considerations must be kept in view. 1. What are the phenomena of vital reaction which appear immediately after the infliction of a burn during life, and remain after death? 2. Do these phenomena appear in every instance of severe burning, even when the person survives but a few minutes, or a single minute? 3. Can they be produced or imitated by any of the effects of burning produced immediately after life is extinct? Such are the points I propose to settle in the succeeding investigation.

1. and 2. Of the effects which follow the application of heat to the living body, the most immediate is a blush of redness to a considerable extent around the burnt part,—removeable by gentle pressure,—disappearing in no long time,—and not permanent after death.—Next to this in order, and occurring indeed most generally at the very same time, is a narrow line of deep redness, separated from the burnt part by a stripe of dead whiteness,—bounded towards the white stripe by an abrupt line of demarcation,—passing at its outer edge by insensible degrees into the diffuse blush already described,—but not capable of being removed, like it, by moderate pressure. This line of redness may be seen very distinctly after the application of the actual cautery, the immediate effects of which represent exactly what may be looked for in a case of speedy death by burning. The redness is obviously caused either by extravasation or very minute capillary injection of the true skin. In every instance in which I have watched the effects of the actual cautery, as well as in the cases which have been observed at my request by others, it appeared in a very few seconds, sometimes in five, generally within fifteen, and once only so late as thirty seconds. I mean, that in this short space of time, the inner edge of the redness surrounding the cauterized part was deep crimson, and incapable of being removed by pressure. I have farther examined carefully this appearance in the bodies of persons burnt

a few hours before death, and never failed to observe it, forming a line on the entire skin near the burn, from a quarter to half an inch in breadth, and about half an inch from the burn.—The next appearance in point of order is blistering. I have not been able to determine the usual period at which blisters are formed. But from the observations I have made, it is obviously an uncertain consequence of a burn, if life be extinguished a few minutes afterwards. When the burning body is a scalding fluid, blisters generally appear in a very few minutes; yet sometimes in very extensive burns of this kind, especially in young children, there is no vesication at all even in many hours. When the burning cause is an incandescent body, vesication is by no means so invariable a consequence as might be supposed. For example, it is seldom witnessed at all round the edge of a burn produced by the actual cautery, probably on account of the circumscribed manner of applying the heat. At the same time, it is certainly often observed very soon after an ordinary burn, such as arises from the clothes catching fire; and the case of the woman in the second trial, proves that it may occur even when the individual is burnt to death on the spot, and consequently does not survive the burn many minutes.—The other vital consequences of burns ensue at too remote a period to be of any use for the object now in view.

3. It follows, then, that the only effects of burns which appear immediately after the injury, and remain in the dead body, are, *first*, a narrow line of redness near the burn, not removeable by pressure, and, *secondly*, blisters filled with serum; that the former is an invariable effect; but that the latter is not always observable when death follows the burn in a few minutes.

Before these appearances can be assumed as indicating that the burn was inflicted during life, it remains to be inquired whether they can be produced or imitated immediately after death, while vitality still lingers in the body, or, to use Bichat's phrase, while organic vitality survives the extinction of animal life. For this end the following experiments were instituted partly by myself, partly by my friends at my request. In every instance the appearances are described from my personal inspection.

Experiment 1*st*.—In a stout young man who poisoned himself with laudanum a very hot poker, and a stream of boiling water, were applied to the skin of the chest and inside of the arm one hour after death. Next day no blisters or redness were visible on or near the burns. At the parts burnt with scalding water, the cuticle appeared as if ruffled, and could be very easily rubbed off; but there was not a trace of moisture on the true skin beneath. At the parts burnt with the poker the whole thickness of the skin was dried up, brownish and translucent, but entirely free of redness or blistering on or around them.

Experiment 2d.—A stout young woman died in ten or twelve days of a low typhoid fever, and at her death was but little attenuated. Ten minutes after death boiling water was poured in a continuous stream on the breast and outside of one of the legs. The body was examined in a day and a half. On the leg no trace whatever could be discovered of the action of heat. On the breast, the place where the water had been poured on it was of a very pale brownish hue, the cuticle slightly shrivelled, dry, brittle, and easily scratched off. The surface of the true skin below was dry; and around the burnt part there was not a vestige of redness or blistering. In this instance the heat was applied so soon after death, that the gentleman who applied it felt convinced he observed the chest heave up when the hot water was poured on it.

Experiment 3d.—A very powerful athletic young man poisoned himself with laudanum; and although the stomach pump was successfully applied not many hours after he had swallowed it, continued completely comatose, and without any sign of sensibility under the ordinary stimulants. Four hours before death a tin vessel, filled with boiling water, was closely applied on several parts of the arms; and a hot smoothing-iron was held on the outside of the hip-joint. Half an hour after death, a red-hot poker was applied to three places on the inside of the arm. The body was examined in thirty-eight hours.

Some of the spots burnt during life presented a uniform blister filled with serum. On two there was no blister; but the cuticle was gone, and the true skin dried into a reddish translucent membrane, at the edge of which there were drops of serum, and also particles of the same fluid dried by evaporation. Round all these spots there was more or less scarlet redness, particularly round the two spots last mentioned: A bright-red border half an inch wide surrounded the whole burns; and the redness was not in the slightest degree diminished by firm pressure.—The spots burned after death were, some of them charred on the surface and not elevated; two presented vesications; but the blisters were filled with air, the cuticle over them was dry and cracked, and the surface of the true skin beneath was also quite dry. On the white parts of the skin there was no adjacent redness. At a part of the edge of two of the burns, however, the lividity which appeared on this, as on most dead bodies, approached very near the margin; but the discoloration could be almost entirely removed by moderate pressure continued for a minute.

Experiment 4th.—Half an hour after amputation of a leg, a cauterizing iron was applied to it. Around the cauterized part whiteness and dryness were produced, but no redness or vesication.

Experiment 5th.—Two hours after death subsequent to amputation of the arm, a cauterizing iron was applied to the remaining arm. The appearances were the same as in the last experiment.

Experiment 6th.—Ten minutes after the amputation of a leg, a cauterizing iron was applied to it. The effects were the same as in the fourth experiment, except that blisters were formed round the burn,—dry, however, and filled with air.

From these experiments it appears that the application of heat to the body even a few minutes only after death, cannot produce any of the signs of vital reaction formerly described. It farther appears that the lividity which follows death in most instances may assume such an arrangement as to imitate the red border produced by a burn during life. But an experienced person can easily recognize the appearance put on by lividity; and if its general appearance should not serve to characterize it, it may at once be known by the effect of continued moderate pressure in removing the redness. It should be understood, then, that, so far as the preceding experiments go, a line of redness near the burn, not removeable by pressure, and likewise the formation of blisters filled with serum, are certain signs of a burn inflicted during life.

ART. VI.—*Observations on Acupuncturation.* By JOHN TATAM BANKS, M. D., one of the Physicians to the Lowth Dispensary, &c. &c.

HAVING a few years ago witnessed in Paris the benefit arising from acupuncturation in rheumatic and neuralgic affections, I was naturally induced, in my own practice, to adopt in such cases the same remedy. The success which has attended it will, I trust, be a sufficient apology for these lines occupying a page of this Journal,—particularly as acupuncturation is, I believe, but little used in England.

Aware that this treatment, under the name of zin-king, is extensively, and has been immemorially, followed in many eastern parts of the world, in acute muscular and nervous pains, I was repeatedly led to try its effects in similar cases; but in no instance was it accompanied with that decided advantage which invariably attended its use in chronic affections. Thus, so far as my experience goes, it accords with Dr Elliotson's and Mr Churchill's observations, that acupuncture is chiefly useful in chronic cases.

The numerous hypotheses respecting the *modus operandi* of this agent, it is not the object of this paper to canvass; but merely to state, as concisely as possible, in a few subjoined cases, that treatment which the writer has universally found effectual; for he can confidently say, that, among upwards of sixty cases of chronic rheumatism, not in one has he failed to afford immediate, and in many permanent, relief by acupuncturation.

1. The first case in which I had recourse to the needles was in a very severe rheumatic affection of the right hip, with excruciating pain in the posterior muscles of the thigh and leg of that side. The whole limb was much wasted, and the muscles extremely flabby. The individual, a robust man, residing at Conisholme, a village about nine miles from Lowth, had been for many years subject to rheumatic pains, but had never, he said, suffered such agony as he had endured the last fortnight,—the whole of which time he had been under the care of an intelligent surgeon in the neighbourhood, but without deriving any benefit, although the usual remedies had been judiciously resorted to,—as bleeding, fomentations, cupping, blisters, stimulating and sedative linaments, Dover's powder, acetate of morphia, &c. When I visited him, he was lying on the bed quite unable to turn himself, with his day-clothes on, and which, from intensity of pain, he had not been able to have taken off for the last three days. When he saw me, extending his hand, and bursting into tears, he begged some means of relief; but could scarcely utter a connected sentence, it being continually broken with cries of suffering.

Seeing at once the nature of his complaint, I did not hesitate to insert five needles into the anterior part of the *glutæus medius* and *maximus*, and four into the *tensor vaginæ femoris*. They had not remained in this situation longer than two minutes, when he declared that the hip was altogether free from pain, and eagerly desired me to puncture the back part of the thigh and leg. I accordingly introduced five needles about an inch deep into the *biceps flexor cruris*, and *vastus externus*, and three into the *gastrocnemius externus*, about an inch and a-half apart. The muscles seemed considerably affected with spasm, but the pain entirely vanished. The man felt certain he could now walk, and in an instant was out of bed, and began striding and jumping across the apartment. During these manœuvres the needles were in the limb, and remained there altogether about fifteen minutes. The operation did not induce much pain, except a little in the *gemellus*. I have always observed that the introduction of the needles into the calf of the leg occasions more uneasiness than in any other muscular part.

It is now many months since this case occurred; and a few days ago I was assured by the individual himself that he had not had any relapse. So confident is he of the certain efficacy of this remedy, that he says he shall never again suffer so much from rhuematism.

2. On my return from visiting Mr Atkinson, the patient whose case I have just stated, I was desired by Mr Welbourn, a farmer of Yarborough, to prescribe for his rheumatism, which, at the time he spoke to me, was, he said, more than usually severe. The pain was chiefly in the legs and hips; and it was with great difficulty he moved from one room to another. He had at different periods consulted many medical men, occasionally deriving benefit from each, but never for longer than a week or ten days together. He said he "had had medicine enough for the whole parish, but would give it one more trial; and if I failed, he would have nothing more to do with it, but rest content with flannel." I could not learn what he had taken except mustard-seed, which at one time he had found serviceable.

When I told him I could, I thought, relieve him in five minutes without his taking a drop of medicine, he imagined I was joking, and replied, "you'll be more successful than I take you for then." He did not, in the smallest degree, object to my using the needles; and allowed me to insert five in the left hip, and four in the left leg, each about an inch and a-half deep. They seemed, upon being introduced, to cause some little pain, and, when stationary, "drew," he said, "like a blister." In less than three minutes the rheumatic pains in these parts had entirely gone; but a metastasis to the *rectus femoris* occurred, where he had never had pain before, but which likewise ceased upon the insertion of one needle. The other leg continuing to torment him, I was anxious to proceed in my work, which he would not permit,—stating that he "had, he thought, for one day had quite enough, and would stop to know how the left leg went on." Twenty minutes elapsed before I removed the needles, which were in a state of oxidation, the first and only time I had noticed the circumstances.

So delighted was he at having one leg and hip free from pain, that, upon my departure, he wavered a good deal whether to have the other done or not; at last he determined to wait and see the result of what had been done. The following week he called on me at Lowth, and, finding he had not experienced any pain since the operation in his left hip and leg, allowed me to acupuncture the other. I did so, and with equal success. He had no relapse, and only complains of slight stiffness and, in some degree, numbness in his joints, which I imagine a second operation would remove.

3. Mr Scott, a carver and gilder in this town, had for many years back, been occasionally harassed with erratic rheumatism; but, for three days prior to my seeing him, he had been affected with acute pain in the lumbar region, shooting down into the *os sacrum.* It was so violent that he could not stand upright, nor walk a yard. He assured me he had not slept for many nights, nor known a minute's ease. He was taking, when I visited him, a terebinthinate preparation, and had tried at different times a variety of internal and topical remedies. Among the former may be enumerated Dover's powder, colchicum, mustard seed, and especially the ammoniated tincture of guaiacum, from which he thought he derived more benefit than from any thing else; and among the latter the compound camphor liniment, blisters, cataplasms of ammonia, mustard, &c. He compared the pain he suffered to scraping the bone with a file, and willingly consented to my inserting half-a-dozen needles in the lumbar region, being satisfied that no pain could exceed his present suffering. While introducing the needles, he complained a good deal of their sharpness, but his next tones were those of gratitude and delight. He did not derive that almost instantaneous benefit which I have frequently seen follow the introduction of the needles; for ten or twelve minutes elapsed before he declared himself entirely free from pain. He could now stand erect, and walk, and bend his body without the least difficulty. A considerable flush or *areola* surrounded the needles; and in extracting them, after they had remained about half an hour, a little force was necessary,—but which caused no additional uneasiness. He went to work the same day. I recommended him to pay particular attention to the state of his bowels, and to avoid a cold or damp atmosphere,—directions to which he has attended. It is several months since this case happened, and without any recurrence of pain in the loins.

4. A tanner in this place had been for two years affected with rheumatism in the right arm and shoulder, and was occasionally unable to move either without extreme pain. Having been frequently told of the relief I had afforded in similar cases, he would long since have applied to me, but was afraid to undergo an operation which idle gossip had magnified as peculiarly dreadful. At length his pain overcame his fears, and he submitted quietly enough to an operation, which he afterwards called but a flea-bite in comparison to the disease. Two needles were introduced into the *deltoides,*—one into the *biceps flexor*

cubiti, and two into the *trapezius*. Some time elapsed before he felt himself easier; but, in the course of eight or ten minutes, he assured me he had not the slightest pain anywhere. The muscles powerfully contracted on the stimulus of the needles, which were a good deal shaken. Since this time (six months) he has followed his business without any interruption from pain.

5. A stout healthy-looking man had been, for nine days, labouring under *neuralgia faciei*, generally named *tic-douloureux*. The symptoms indicated the disease to be seated in the *portio dura* of the seventh pair of nerves of the right side, the divarications of pain corresponding exactly with its course and communications. The muscles of the face were affected with convulsive twitchings, and he appeared to suffer the most pungent agony. But he complained most bitterly of deep-seated pain at the under and back part of the ear, where the *portio dura* passes out of the bone through the *foramen stylo-mastoideum*. The pain came on in paroxysms. During the first three or four days a paroxysm usually occurred about six in the evening, and continued violent between two and three hours; but latterly they had recurred so frequently, during both day and night, as scarcely to leave him the enjoyment of half an hour's ease. He attributes his complaint to getting his feet wet, and remaining too long in his wet shoes and stockings. On my first seeing him, he strenuously refused to have the needles inserted, and wished me to prescribe some medicine. In the present instance the brain, I fancied, did not escape disease; for weight and pain were felt in the head, with slight giddiness, the pulse was full and accelerated, and the bowels regular. Conceiving the *neuralgia facialis* might probably proceed from determination of blood to the brain, I directed bleeding, both general and local, and aperients, together with a blister to be applied on the back of the neck. By these means the head was entirely relieved; but the anguish produced by the *tic-douloureux* continued as great as ever. I now prescribed one drachm and a half of the subcarbonate of iron four times a-day, and persisted in its use two days, but without the least abatement of the symptoms. I next ordered the extract of *stramonium*, but without advantage; and did not succeed in allaying the pain until I had recourse to *belladonna*, from which medicine he certainly derived, for some days, great benefit. But a paroxysm of most acute pain suddenly occurring when I happened to be present, quite disheartened him; and he was now, upon my relating one or two cases in which I had seen acupuncture serviceable, readily induced to try its effects on his own person. I accordingly lost no time in introducing five needles in the se-

veral directions where the pain darted, in the right side of the face and temple. Before I had well finished doing so, he joyfully exclaimed, that the pain of his face had completely gone. On the following morning, when I called, he told me he had suffered no pain the whole night; and appeared so satisfied that he owed his present ease to the good effects of the operation, that he earnestly begged of me to repeat it. It may appear strange to say, that I did so merely to gratify him; but such was the fact. Although three months have transpired since this case was under my care, yet no tendency whatever to this excruciating disease has again manifested itself.

In no instance have I known any untoward symptom occasioned by acupuncturation. I cannot affirm that no pain is produced by the operation; but certainly it is very slight. Where a syphilitic taint is connected with rheumatism; of course acupuncturation alone will prove unavailing.

December 13, 1830.

ART. VII.—*On preserving Anatomical Preparations in Spirits.* By ALEXANDER WATSON, Esq. Fellow of the Royal College of Surgeons, Edinburgh, &c. &c.

NOW that museums of healthy and morbid anatomy are considered to be of so much importance, it is surprising that more attention is not given to the improvement of the mode of putting up preparations in spirits. This mode of preserving anatomical and pathological specimens is of great importance, as it is the most perfect, though it is, at the same time, by far the most troublesome and expensive. It becomes interesting, therefore, to inquire what is the best mode of securing preparations of this class from evaporation and decay.

As the "tying-up," as it is called, of preparations in spirits, is generally left to be done by those who know very little about the matter, and care only so far as they are paid for their labour, they either cannot or will not improve upon an old, troublesome, and very inefficient mode of doing it. They therefore do not use any endeavour to discover the causes of its defects. In consequence of having experienced its defects, I some time ago endeavoured to ascertain, 1*st*, What were the chief causes of the evaporation of the spirits from preparations which had been put up in the most careful manner, by tying over the glass layers of bladder, sheet tin, and superadding coatings of varnish; 2*dly*, To contrive a mode of putting up wet preparations, that would obviate the chief objections to the old methods. And, though my conclusions may not be altogether new, yet, as they may be interesting to those forming museums, I communicate them through the medium of your Journal.

1. When an anatomical specimen is suspended in a preparation glass, according to the present ordinary mode, the threads suspending it are necessarily brought over the edges of the glass, moistened bladder is tied over it, then a covering of sheet tin, and another covering of bladder, which, when dry, is varnished over. Now, though the glass is tied up perfectly full and free from globules of air, at the end of a year or two, and often much sooner, the spirits have so much evaporated that the preparation requires to be re-tied up. This is not only the source of much trouble, but delicate preparations are often much injured, or even altogether destroyed, in undergoing this operation.

The evaporation of the spirits seems to take place chiefly from being exposed to considerable variations of temperature. After a preparation has been tied up, particularly in winter, or in a cool room, and then placed in a warm apartment, or exposed to the summer heat, a very considerable expansion of the spirits takes place. The force of this expansion causes the formation of some small crevice or external opening between the glass and its covering, very often by the side of the thread suspending the preparation. When such an external communication is once formed, the evaporation of the spirits goes on by the ordinary variations of temperature to which it is exposed, more or less quickly, according to the degree of these variations,—air is admitted when the temperature falls, and upon another increase of temperature, the fluid is wasted by the air being expelled carrying the spirits along with it. This process by repetition goes on till the preparation is left in the glass quite dry.

When a preparation is tied up in summer, or in a warm apartment, and then exposed to a lower temperature, the change produced upon it at first is different, but not less destructive to it than that in the former case. When the contents of the bottle contract by the lower temperature, the covering is forced down into it by the pressure of the atmosphere. By this forcible depression of the upper part of the covering, its sides are drawn upwards, and some chink or crevice is formed, by which air is admitted. After this has taken place, the waste of the spirits goes on, as in the preceding case, by every increase of temperature.

2. In order to obviate these inconveniences, I thought of the following mode of putting up preparations. I obtained preparation-bottles, with glass stoppers accurately ground to them. When these are intended to contain small or round prepara-

tions they may be made somewhat globular, (as Fig. 1;) a form which is both more ornamental, and contains less spirits than any other; consequently, there will be less expansion, and the mouth of the bottle being narrowed, exposes less surface to evaporation; besides, the globular form magnifies the preparation very considerably, which in many cases is a great advantage. For other preparations, bottles of other forms will of course be more suitable,—attention always being paid to having the mouth of the bottle of as small dimensions as just suffices to admit the preparation. Thus a membranous preparation of three or four inches square, having its upper part attached to a piece of glass rod, may be introduced into a bottle sufficient for its expansion, at an aperture of about half an inch in diameter, (as Fig. 3,) the suspending thread being attached to the glass rod. Solid preparations require the mouth of the bottle to be nearly of the same width as its interior, (as Fig. 2.) The bottles can, of course, be made of any depth; they could also be made flat-sided with a circular mouth. The hollow form of the stoppers of these bottles possesses several essential advantages. *1st*, It admits of a small piece of whalebone being placed across its interior, to which the preparation is suspended, by which the necessity of threads hanging over the edges of the bottle is obviated. *2d*, It admits of a portion of the air contained in it being expelled by the application of a moderate heat before being put on. This is of very considerable importance. When the stopper is thus firmly placed upon the bottle, being perfectly air-tight, it is forcibly retained in its place when cooled by the pressure of the atmosphere; and the partial vacuum within it, admits of the expansion of the spirits to a considerable extent before the stopper could be forced up, or any of the spirits forced out. *3d*, An accurately ground glass stopper forms a very easy and expeditious mode of closing preparation-bottles, thereby saving manual labour. *4th*, It looks much neater and more elegant in a museum than the tying up with bladders, &c. *5th*, When evaporation does take place, the refilling of the bottle only requires the stopper to be raised a little with one hand, while some spirits are poured in with the other; thus saving all trouble, expence, and risk of injuring the preparation necessarily incurred by the old and filthy bladder mode.

I have shortly described the mode which I have adopted, and would recommend others to adopt, in the making of wet anatomical preparations. But, before concluding, I may notice two objections which may be started to this method. *1st*, The greater expence of bottles with ground stoppers than those now used. From experience, I can assert that, when the preparation-glasses, bladders, sheet-tin, twine, varnish, and manual labour, are taken into account, the adoption of stoppered bottles will be found a considerable saving, even without taking into account the expence and trouble of refilling, which so soon becomes necessary, and which, if considered, would prove the new method to be a positive saving, independently of its other advantages. *2d,* The unfitness of this method for preparations of a large size. There is no doubt that for very large preparations, or those of a peculiar shape, such bottles could not be adopted. But I know that stoppers can be accurately ground to bottles whose mouths are four inches in diameter. I will therefore venture to say, that about three-fourths of the preparations of a museum might be put in bottles made in this manner.

Another circumstance of great importance in preventing evaporation from preparations, besides those already mentioned, is to use spirits as weak as is consistent with the preservation of the part. When the part has been properly macerated and prepared by repeated changes of water and spirits, a much weaker spirit than is commonly used is not only sufficient, but is in reality better for preserving it, by being less subject to evaporation. The stopper should also be rubbed over with a little lard before being inserted into the bottle, and melted bees-wax afterwards poured round it.

Fig.1

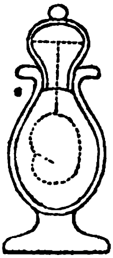


Fig.2

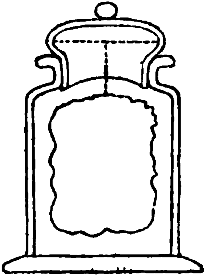
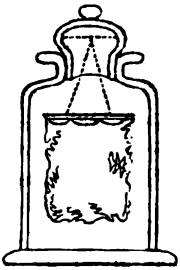


Fig.3



Art. VIII.—*On a process for preparing economically the Muriate of Morphia.* By William Gregory, M. D. Fellow of the Royal College of Physicians, Edinburgh, and Lecturer on Chemistry. *With a Letter from Dr Christison on its employment in Medicine.*

In a note added by Wöhler to the last edition of Berzelius's Lehrbuch, several processes are given by Wittstock for the separation of morphia from narcotine, which is not completely effected by the ordinary process. One of the processes consists in treating the crystals of morphia, supposed to contain narcotine, by muriatic acid in slight excess, which dissolves both, and evaporating until a mass of feathery crystals of muriate of morphia is deposited. The crystals when freed from the adhering liquid by pressure between folds of bibulous paper, are pure muriate of morphia; the muriate of narcotine being retained in the mother liquid. This process, founded on the fact that muriate of narcotine does not crystallize, I have found to answer very well; but the author directs it to be employed on the crystals of morphia, of course obtained from the alcoholic solution. It struck me that it might be applied to precipitated morphia, and that thus the use of alcohol, so expensive in this country, might be avoided. As the muriate of morphia promised to be a salt useful in medicine, I endeavoured to find a method by which as large a quantity as possible might be extracted from opium *without the use of alcohol*. After many trials, the following process appeared to answer my expectations.

Opium is cut in small pieces, and completely exhausted by cold water, or water at 90° F. The aqueous infusion is concentrated till it occupies a small bulk, and precipitated by a slight excess of ammonia. The precipitate is collected on a filter, washed moderately with cold water, and dried at a temperature below 212°. When dry, it is reduced to powder, and rubbed up with cold water. Diluted muriatic acid is now added by degrees. The first portions are speedily neutralized, but fresh acid is added until a slight but permanent excess is present. This dissolves both the morphia and narcotine, forming a dark-brown solution, which must be filtered to separate it from some very dark matter which is left undissolved. The filtered solution is now evaporated to nearly the consistence of syrup, and on cooling forms a brown mass of crystals moistened with a very dark liquid. The whole mass is now subjected to strong pressure between folds of bibulous paper, which absorbs the liquid containing the muriate of narcotine and colouring matter, and leaves the muriate of morphia tolerably pure, although still of a brownish colour. A second solution, crystallization, and expression, yields the salt nearly white and free from narcotine. By a third crystallization the muriate of morphia may be obtained in radiated bunches of silky crystals of snowy whiteness. These crystals, when dried by a moderate heat, become quite opaque. They are soluble to almost any extent in boiling water. Their solution has a very bitter taste, and yields, when supersaturated by ammonia, a highly crystalline precipitate of morphia. A similarly pure solution of narcotine in muriatic acid gives a curdy precipitate not at all crystalline.

The quantity of muriate of morphia obtained from opium by the above process varies according to the quality of the drug. From a very pure specimen of Mr Young's British opium I obtained 13.5 per cent of nearly pure salt in one of the first experiments I made. I have no doubt there was here considerable loss. On the other hand, from an equally pure specimen of East India opium, I obtained only 4.5 per cent. Here also I believe a good deal was lost.[*] The average amount I have hitherto obtained from good Turkey opium is from 10 to 11 per cent., quite pure. A great loss is sustained in the purification. Thus, in the experiments which yielded the above average quantity, the amount of the impure salt obtained by the first crystallization, which had only a fawn colour, was always from 13 to 15 per cent. From a beautiful specimen of Turkey opium, I even got 17.5 per cent. of the impure salt. In all cases, the salt was dried by a heat of 150° F. until it ceased to lose weight, before its quantity was ascertained. In the repeated processes of expression, much muriate of morphia is absorbed by the paper along with the muriate of narcotine, and it may even be again recovered from the paper. I am at present engaged, along with my friend Dr Montgomery Robertson, in endeavouring to purify the muriate with a smaller loss, and we have good hopes of the result. But, taking the quantity of pure muriate obtained at 10 per cent., that is, about 12 drachms from the pound avoirdupois, this is incomparably the cheapest preparation of morphia having any pretensions to purity. The acetate used in this country, and the sulphate employed in France, are made, or should be made, by combining crystallized morphia with the acids. But, to obtain the morphia crystallized, a quantity of alcohol must be employed, which, in this country at least, enhances very much the expence of the preparation; and it is this expence which the above process will obviate, as no alcohol whatever is employed.

Since making the experiments which have led me to recommend the process here described, I have observed that MM. Henri and Plisson had previously published one in some respects similar, founded on the *insolubility* of narcotine in muriatic acid, when very much diluted. They advise that the opium be digested at a temperature between 90° and 104° in diluted muriatic acid,—that the infusion be precipitated with ammonia, and the mother-water acidulated, concentrated, and precipitated again with the same reagent;—that the impure morphia thus procured be next dissolved with successive portions of very dilute muriatic acid till there is a faint excess, by which the narcotine is left, and the solution subsequently evaporated and crystallized;—that the muriate of morphia be lastly washed with a little water, and purified by animal charcoal and crystallization, successively repeated twice or thrice, according to circumstances. They then direct the morphia to be precipitated by ammonia from a solution of the pure muriate made with a faint excess of muriatic acid.[*] The peculiarities of the process I recommend, which give it in my opinion a considerable advantage over that now stated, are that in the first instance water only is used for preparing the infusion, by which less of the resinoid matter is taken up, while all the morphia is dissolved; and that instead of purifying the muriate of morphia by washing and the repeated use of animal charcoal, both of which steps involve a considerable loss, and cannot entirely remove the narcotine, I accomplish the same end more economically, by simple expression and crystallization. To the loss sustained by repeated washing and digesting with charcoal, and not, as MM. Henri and Plisson imagine, to mere inferiority in the sample of opium they used, I am inclined to ascribe the small proportion of morphia procured by their process,—amounting, in fact, to no more than 6½ per cent.

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[*] According to my experiments, and those also of Dr Christison, East India opium appears to contain little morphia, but abundance of narcotine. The very dark colour of this opium coincides with an observation I have frequently had occasion to make, that narcotine and the resinoid colouring matter of opium adhere very strongly together, and are often found associated.

Not only is the acetate of morphia, the salt usually employed in this country, a very expensive preparation, it is also a very impure one in many cases. The strongest proof I can give of this is, that a quantity of acetate of morphia is said to be prepared in Glasgow, for the London market, from the refuse or marc of laudanum. Now, when the laudanum has been properly prepared, I find that the marc contains no morphia, but a good deal of narcotine; and the French chemists are aware of this, so that they have recommended to extract the narcotine from the marc by means of acetic acid. Even if the opium used for laudanum be imperfectly exhausted, which is no doubt sometimes the case, any morphia which may remain in it must be strongly charged with narcotine, and hence unfit for internal use. It is impossible, except on the supposition of impurity, to account for the frequently unsatisfactory results, in this country, of the administration of remedies so well established abroad as the salts of morphia.

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[*] Journal de Pharmacie, xiv. 252. 1828.

Lastly, the muriate of morphia seems to me to keep perfectly, whether dry or in solution.

From the facts I have mentioned, I was led to prepare a considerable quantity of the muriate of morphia, for the purpose of having it fairly tried; and I have it in my power, by the kindness of Dr Christison, to offer to the public the results of some experiments made by him at my request. These results I give in Dr Christison's words, as communicated to me in a letter dated the 11th February 1831.

The solution employed by Dr Christison was as follows.

℞ *Muriatis Morphiæ, gr. x. Aq. calid. Min.* 1000. *Solve.*

Of this solution, 100 minims contain 1 grain, and 25 min. ¼ grain. A more convenient formula, and nearly of the same strength, is 5 grains of salt to 1 ounce of water. Such a solution, I am convinced, might be made as cheap as laudanum, while it is more than equal to laudanum in anodyne power.

"My Dear Sir,—I have administered the solution of muriate of morphia in upwards of thirty cases, and in several of them for ten or fifteen days continuously. The dose has been, with very few exceptions, twenty-five drops of the solution, corresponding with a quarter of a grain of the muriate. In every instance sleep supervened in the course of an hour, and continued between four and seven hours. In by far the greater number it was profound and calm, without dreaming; in three instances there was much dreaming, but always of a pleasant nature, and followed by the refreshment of natural sleep.

"The after-effects of this preparation have appeared to me invariably trifling. In scarcely a single instance has its employment been followed by dry brown tongue, sickness, or headach, which are so frequently induced by opium even when it produces refreshing sleep. In one case only, I remarked a dry brown streak along the centre of the tongue in a gentleman who took it for the sixth evening; but in this, as in other instances of the same kind, a habitual sickness after meals in the morning was lessened rather than increased; and although there was previously to the use of the muriate of morphia frequent hemicrania, he never had headach of any kind after using it. In short, the taking of this remedy appears, so far as my experience hitherto goes, entirely free of the bad effects produced by opium on the stomach, after its soporific influence ceases.

"I have not as yet had any opportunity of trying its effects in a case where opium acted deleteriously; but on several occasions I have compared the effects of the muriate of morphia on the one hand, and of solid opium, laudanum, Battley's sedative liquor, and a new East India laudanum of great strength, on the other. To some patients I have given the salt of morphia, after they had been in the practice of taking laudanum for weeks continuously to the amount of forty or fifty minims. In all, twenty-five drops of the solution produced equally sound sleep; the greater number declared they slept more uninterruptedly, and awoke more refreshed; and a few, who had suffered sickness and headach in the morning when they took laudanum, were free from these inconveniences after taking the morphia. To other patients who had taken laudanum or Battley's sedative liquor for some time with apparently good effect, I first gave the muriate of morphia for two evenings, and then administered again the preparation of opium previously taken. In every instance of the kind but one, I was requested by the patient to resume the muriate of morphia, which I was assured gave him a sounder sleep, and left him in a state of greater general comfort in the morning. One patient, a female, first preferred the muriate of morphia, but afterwards requested to resume the use of laudanum, which she said made her sleep better. But she was so capricious in many other respects, that no importance can be attached to this apparent exception.

"I think I have also remarked two other circumstances in the action of the muriate of morphia, which, if they shall be confirmed by future experience, cannot fail to give this preparation a great advantage over the other preparations of opium. The first is, that while its soporific effects appear, on the one hand, more complete for some hours after its administration, so, on the other hand, they seem to pass off more suddenly and completely than those of laudanum and Battley's solution. Every one who has extensively observed the effects of the last-mentioned preparations must have remarked, that in many instances the patient continues torpid and drowsy during the whole forenoon, after having slept even soundly the previous night. This I have never had occasion to remark in any of my patients who had taken the muriate of morphia. The second circumstance, if my observations are correct, is even more important; and it is, that the muriate of morphia, after being taken many days in succession, still continues to act with equal activity without being increased in quantity. Some of my patients continued to take it for fifteen days every evening without intermission, in the dose of twenty-five drops; and to the last they slept the same number of hours, and as soundly as at first. That their repose was not owing to natural sleep was proved in some by subsequently intermitting the morphia;—for then the patient passed a sleepless night.

" The muriate of morphia has appeared to me to possess the same inconvenience as the other preparations of opium in constipating the bowels.

" From my whole observations, I have no difficulty in coming to the conclusion, that the muriate of morphia is a much superior preparation of opium to any other in which the morphia is not reduced to the state of a simple pure salt. This is a much more favourable result than others have come to in regard to the acetate of morphia; and indeed I may add, that I have myself several times found the acetate to act imperfectly as a hypnotic, and to induce subsequent disorder of the stomach. This, however, can scarcely be imputed to any intrinsic inferiority of the acetate to the muriate; and may be ascribed with much more probability to the extremely careless way in which the acetate is habitually prepared. Till within these few months the acetate sold in the shops of this city was brown in colour, and formed a brown solution,—owing to the salt not having been deprived of the resinoid or colouring matter; and it is far from improbable that this resinoid matter is in some way or another connected with the deleterious effects which most of the preparations of opium produce on the stomach.

" Considering the great simplicity of your process for preparing the muriate of morphia, and its salutary action as a remedy, to which may be added the facility it gives of administering a preparation of opium without the patient discovering it by the taste, which is purely bitter, I certainly entertain sanguine expectations that it will soon come into general use.—Yours, &c.

" *Feb.* 11*th*, 1831. R. C."

Several other practitioners in this city, among whom I may mention Mr Liston, have, at my request, made trial of the muriate of morphia, and have obtained analogous results to those here related. Practitioners who may wish to satisfy themselves of its efficacy, may procure it in a state of purity at Messrs Duncan and Ogilvie, or Messrs Cheyne of the Apothecaries Hall, Edinburgh.

At present, as the salt is only made by chemists on a small scale, its price may appear to some a sufficient reason for preferring the other preparations of opium, though less efficacious and salutary in their action. This objection, however, will be completely removed as soon as the chemical manufacturer shall prepare it on a large scale. The wholesale price of an avoirdupois pound of average opium,—which according to my experiments will yield an ounce and a-half Troy of extremely pure muriate of morphia, and on a large scale, probably a drachm or two drachms more,—has been for the last two years, on an average, twenty-five shillings. If we add 25 per cent. to this for expence of process and profit to the manufacturer, which is sufficient for an article that must cost very little in apparatus or materials, there will be procured an ounce and a-half for thirty-one shillings and threepence; and this quantity makes, according to the experiments of Dr Christison and others, 2880 full doses.

Let us now compare the expence of this preparation with that of laudanum and the Sedative Solution of Battley. For Battley's solution the manufacturer charges the exorbitant wholesale price of twenty shillings per pound avoirdupois. Now, its medium dose, according to the experience of many practitioners here, and especially that of the physicians of the Infirmary, being twenty-five minims, and its density almost exactly that of water,—a pound avoirdupois, that is 7000 Troy grains, or 7375 minims, will give only 295 doses. The expence of a dose of Battley's solution is therefore six times that of a dose of muriate of morphia.

By a similar calculation, it may be proved that the muriate of morphia will be even somewhat cheaper than common laudanum. To convert an avoirdupois pound of opium into laudanum, the Edinburgh Pharmacopœia directs it to be infused with twelve pounds of proof spirit, which is made with equal parts of water and alcohol of 835 density. Now the wholesale price of alcohol of this strength being in Scotland eleven shillings the gallon, and the gallon containing 8.35 avoirdupois pounds of such alcohol, the six pounds required to convert a pound of opium into laudanum cost nine shillings and twopence. So that a pound of opium made into laudanum by the Edinburgh process, costs the apothecary thirty-four shillings and twopence. The quantity of laudanum procured by this process is about a twelfth less than the amount of proof spirit used, that is eleven pounds,—a portion being retained by the mass of opium, however well squeezed. As the density of the laudanum is 920, the eleven pounds will give 87087 minims. According to the experience of many professional friends here, a full dose of laudanum is at least thirty minims. Hence for thirty-four shillings and twopence there is procured 2903 doses.

Expressing these data in simpler terms, it appears that for twenty shillings the apothecary should receive 295 doses of Battley's solution, 1700 doses of laudanum, and 1840 of muriate of morphia. The muriate of morphia is at once, then, cheaper and more efficacious than any of the preparations of opium now in general use.

ART. IX.—*On the Analysis of Elaterium, and its active principle.* By JOHN D. MORRIES, Esq. Member of the Royal Medical and Plinian Societies of Edinburgh. [*]

THE great uncertainty of the effects of Elaterium as a cathartic, and the probable dependence of this uncertainty on original differences in the quality of the drug, as well as on the occasional addition of impurities, render it an object of much consequence to determine what is its active principle, and how this may be separated in a pure state. Several attempts have already been made to attain this object,—among which it is only necessary for me to particularize the analysis of Dr Paris, first published in 1822. According to this analysis, the active principle of elaterium, which he terms *Elatine*, is a green, soft, inflammable matter, free of bitterness, insoluble in water, and soluble in alcohol and the alkalies. The action of this substance as a medicine was so violent as strongly to favour the idea of its being the active principle. But a variety of circumstances tend to throw doubt over the accuracy and completeness of Dr Paris's analysis. Among these, it might be sufficient perhaps to observe, that he operated on so small a quantity as ten grains only of elaterium,—a quantity which could scarcely be expected to yield satisfactory results. But besides, in the physical and chemical characters of the substance procured by Dr Paris, the chemist must recognize a close resemblance to chlorophylle, and certainly no analogy with the active principles which have been discovered both before and since in almost every plant possessing an action on the animal body.

On repeating Dr Paris's experiments on the same quantity which he employed, I found results differing materially from his. Of ten grains of apparently good elaterium he found four soluble in water;[†] but on repeating this, I found that water only dissolved two grains and nine-tenths. Of the six grains insoluble in water, he found only five-tenths were dissolved by alcohol;[‡] while I found that alcohol took up two grains and two-tenths.

These discrepancies of result must have arisen from the very great difference which prevails, as will presently appear, between various specimens of elaterium. But on proceeding with the analysis, I remarked other more important differences.

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[*] In some of the London weekly journals, published on the 12th February, the discovery of a crystalline principle in Elaterium has been announced as made by a Mr Hennell of the Apothecaries' Hall there. In reference to this announcement, we have authority for stating, that the principle discovered by Mr Morries, which is probably the same with that found by Mr Hennell, was put into the hands of Dr Christison early in November, and mentioned a day or two afterwards to various persons in Edinburgh. His paper was read before the Royal Medical Society on the evening of Friday the 4th of February.—[EDITORS.]

[†] Pharmacologia, p. 203. Exp. A. [‡] Ibidem, p. 204. Exp. D.

When fifty grains of good elaterium were digested for twenty-four hours in distilled water at a temperature of about 200° F. they were found to have lost eleven grains. The infusion was of a light brown colour, and an austere bitter taste, and appeared to contain a minute portion of gum, but no starch. On standing for some time a few crystals of sulphate of lime were deposited. The residual quantity, amounting to thirty-eight grains, (one grain having been lost by adhesion to the filter, and other unavoidable circumstances,) was acted on by alcohol, s. g. 825, and was found to have lost ten grains. The alcoholic tincture was of a deep green colour, and a bitter, somewhat acrid, taste. On being evaporated to the consistence of oil, and allowed to cool, numerous masses of small spicular crystals were observed; these were separated, washed with sulphuric ether, and dried. The rest of the extract was boiled in *aqua potassæ*, to free it from the elatine or colouring matter, (terms I consider as synonymous,) which is soluble in alkalies. After standing for a few minutes, a small quantity of white crystalline matter fell to the bottom of the flask. The supernatant fluid was now decanted, and the crystals collected on a filter, and washed with sulphuric ether, to free them more completely from adhering colouring matter.

Having obtained an appreciable quantity of crystalline matter, I proceeded to examine its properties.

It is, as I have already stated, a white crystalline substance of an extremely bitter and rather styptic taste, insoluble in water and the alkalies, soluble in alcohol, ether, and in hot olive oil, sparingly soluble in dilute acids. When procured in a state of purity, as by the process to be described presently, it forms microscopic rhombic prisms, striated on the sides, possessed of considerable lustre, and of a silky appearance when in mass. It is decomposed by the strong acids, forming with nitric a transparent yellowish gummy-looking mass, and with sulphuric, a solution of a deep blood-red colour, which I should rather think caused by adhering colouring matter. It is fusible at a temperature a little above that of boiling water, and at a higher temperature is dissipated in thick whitish pungent vapour, having rather an ammoniacal odour.

Considering this substance as the proximate principle of *elaterium*, I first named it *elateria;* but as its alkaline reaction is not by any means distinct, and it does not appear to exist in the crude drug in the form of a salt, as every one of the acknowledged alkaloids does,—I have since altered it to *Elaterine.*

The process which I at first followed for procuring elaterine was that of washing the small crystals above-mentioned with

r; this I found unmanageable, as the sulphuric
s both the elaterine and colouring matter with

process I tried was by boiling the alcoholic ex-
a potassæ; this also I found inconvenient and
The process I now follow is simple and economical.
lic tincture is to be evaporated to the consistence of
while still warm, is to be thrown into boiling distilled
ediately a copious white crystalline precipitate falls,
ses in quantity as the liquor cools. This precipitate
rine, the elatine remaining in solution. The elaterine
be separated by decantation and filtration, and repeat-
hed with distilled water. In this state it is sufficiently
ordinary purposes and for medical use. If it be required
v pure, it is only necessary to repeat the solution in al-
nd precipitation.

ould propose the same process for all vegetable crystal-
principles insoluble in water, as the separation from co-
g matter is more complete after one precipitation than after
ed crystallization from alcohol in the ordinary method. I
tried it with strychnia, and found it completely successful.
n cold water is used, the crystals are smaller, and do not
ss lustre.

s elaterine appeared to me the active principle, I tried its
ct on animals.

o one rabbit I gave a tenth of a grain in the form of pill,
ich, at the expiration of twelve hours, produced little effect,
cept some tenderness of abdomen. Twenty-four hours after
e first dose, I administered a second in solution, and in six
urs afterwards the animal appeared in great pain, having la-
orious breathing, and other symptoms of general inflammation.
t eleven at night, thirteen hours after the second dose, and
thirty-seven after the first, it died, having neither passed feces nor urine during the whole period.

On opening the body some hours after death, the stomach was found nearly empty and much enlarged, with increased vascularity towards the pylorus; the orifice was contracted, and the whole mucous membrane was softer and more vascular than natural. The lungs were much inflamed, and in some spots transparent and pulpy. The rest of the viscera did not seem at all affected.

The next rabbit to which I gave elaterine had no evacuation for three days; at the end of which period I gave it a fifth of a grain, which proved fatal in two days. In the course of the second day it passed a considerable quantity of milky urine and some feculent matter.

The only morbid appearances were increased vascularity and thickening of the coats of the stomach, with softening of the mucous coat. The lungs were not so much altered as in the first rabbit, but were obviously inflamed to a considerable extent.

The effects on man are similar to those of elaterium itself, viz. increased secretion of urine, nausea, vomiting, and fluid stools.

To a person in perfect health a twentieth of a grain was given, which at the end of two hours produced vomiting and copious dejections. In the Royal Infirmary it was tried by Dr Christison in four cases, in doses of a tenth of a grain. In two of them vomiting and purging were produced, in a third griping, and in a fourth no effect. In these cases I attribute the uncertainty of its effect to its having been made up in the form of pill, as, from trials since made by Dr Duncan in the clinical wards of that institution, with an acidulated solution, a twelfth, or even sixteenth, of a grain has been found a sufficient dose for an adult.

The formula which has been found to succeed most completely is the following:—

℞ *Elaterinæ, gr. i. Alcohol ℥i. Acidi Nitrici, gtt. iv. Solve. Sumat. æ. ʒss. ad gtt. xl. in aquæ Cinnamomi ℥ss.*

In a case of anasarca, I had an opportunity of trying its effects, and found it at least a very useful purgative, the patient always expressing himself much relieved after its operation.

The following table gives a view of the different solubility of different specimens of elaterium, first in water, and then in alcohol, and of the quantity of elaterine contained in each:—

Grs.	British Elaterium.	Soluble in water.	Soluble in alcohol.	Quantity of Elaterine.
200	No. 1,	53	44	30
	2,	50	43	Not estimated.
	3,	44	46	40
	4,	46	45	36
	5,	52	63	52
Grs.	French Elaterium.			
200	light colour.	94	17	Not correctly estimated
	dark colour.	140	20	

The best specimen of the British elaterium contained nearly 26 per cent. of elaterine, and the worst, No. 1. about 15. The French elaterium, which I obtained through the kindness of Dr Duncan, did not appear to contain above 5 or 6 per cent. There is obviously, therefore, an extreme difference in the strength of this drug in its crude state; so that it ought never to be administered in that form, especially considering that its active principle may be procured by so simple a process.

Art. X.—*Observations on Tetanus Infantum, or Lock-jaw of Infants.* By John Hancock, M. D.

THIS disorder is commonly called in the West Indies *Trismus* or locked jaw; but the term is not appropriate, for it consists not only in a spasm of the muscles of the jaw, but is, in every case which I have seen, a universal spasm or rigid contraction of every muscle of the body.

It happens to new born infants under nine days, and seldom attacks them at a later period. It is so frequent and fatal in the colonies of Essequebo and Demerara, that at an average estimate, it kills half of the whole number of infants which are born there; and I have been informed that nearly the same proportion is destroyed by it in the neighbouring islands.

From what I have been informed, it appears to me that the use of cold water is an almost certain prophylactic against the invasion of this disease in the new born infant. It was formerly pursued on Mr Semple's estate, the Brothers, on the east coast Essequebo, under the care of an Indian woman, who acted as midwife, &c. They were in the habit of dipping the infants in cold water, and of throwing it over and washing them therewith, immediately after birth, and repeating it once or twice a-day, without the occurrence of the disease in a single instance for many years. I can also say from my own knowledge, that the native Indians universally employ cold water with their infants as described above, and I have never heard of an infant among them that had the infirmity in question.

This disorder has baffled the skill of every medical practitioner who has hitherto attempted its cure, at least in ninety-nine cases out of a hundred; but now that such an easy preventive is known, it is to be hoped that in future recourse will be had to means so extremely simple, and which are in the power of every one to apply.

The chief and essential cause of lock-jaw in infants is an irritative impression made on the nervous system by the compression of the umbilical chord, caused by the ligature which is applied to it at birth,—a custom which, to say nothing of its destructive tendency, is entirely unnecessary and uncalled for.

It is one of those useless customs which has risen from the doings of old women, or officious accoucheurs, who fancy that nothing can be well done unless they put their hands to it, and most practitioners treading in their steps follow their example, to the destruction of thousands. This custom took its rise in a northern climate, where it is scarcely ever attended with the mischievous effects which so commonly follow its practice between the tropics. Those who give the matter due consideration, and reflect on the evil results which ensue from the continuance of so pernicious a custom, will surely never censure those who discontinue it in a country where it is attended by so many instances of fatality. It is never practised among the aborigines of the country, and I have known no instances of *trismus infantum* amongst their infants.

To these arguments it may be objected, that the navel-string soon loses its vitality, and frequently drops off before the tetanic symptoms appear; and it may be asked, how, in cases of that sort, can such be the cause of the malady? To such an objection as this, which, by the way, is extremely futile, I reply, that the irritative impulse, which is early communicated to the system, remains dormant for a certain time, more or less according to the sum of the exciting or secondary causes, viz. the natural susceptibility of the infant, with the presence of various agents, as smoke, heat, tight bandages, and improper treatment of any kind—all tending to promote and excite the first efficient cause, and bring it into action. In short, it is not necessary that the primary agent should continue to act: it is sufficient to show, that it gives the first irritative impulse, which is brought to a crisis by the other causes before-mentioned.

We know that in cases of hydrophobia the virus of the rabid animal remains dormant in the system until by degrees it extends its malignant influence, becomes active, and manifests itself in those alarming symptoms which betoken the hopeless state of the unfortunate sufferer. Why then may not other maladies to which the human frame is subject owe their origin to causes which do not immediately show their effects? Again, in traumatic tetanus, or locked jaw from wounds, how often does it occur that the disorder does not appear in the person till the wound is perfectly cured?

A sympathy does and must exist wherever there is any circulation of the vital fluid; and it seems, that where the circulation is but partial, as in tendinous parts, the nerves are more subject to irritation on being wounded, and this may account for locked jaws happening more frequently from hurts in the feet. As like causes produce like effects, the same thing would happen were any other part of the body having a similar degree of vitality wounded or hurt in a similar manner; and as it is well known that this degree of vitality exists in the chord after the birth of the infant, it follows, that any injury done to it may be attended with the same distressing consequences.

The umbilical chord of the nascent fœtus, we have reason to believe, from the researches of Sir Everard Home, is supplied like other organic parts with nervous fibrillæ. If, in tying the cord, the ligature be not drawn sufficiently tight to cut off completely the sympathetic communication, an irritation affecting the whole nervous system may be the consequence, especially if cold or other exciting causes, such as smoke, impure air, &c. should be present. The sensibility of these nervous fibres is blunted or destroyed by the application of powerful stimuli to their extremities, as spirits of turpentine, alcohol, &c. The practice of applying turpentine, laudanum, &c. to the cut end of the cord, presupposes an irritability or susceptibility to impressions in the portion of cord remaining attached to the child, for any applications to a dead inorganized substance would be entirely futile. It is not to be doubted, then, that there is for some time after a sympathy existing in this extraneous or caducous part with the living principle in the child, in other words, that it participates with the vital functions.

The application of turpentine, on cutting the cord, is represented by some to be a preventive of lock-jaw ; it may, when other circumstances are favourable, have some influence, but is not alone worthy of much confidence, as experience has fully proved.

May not the locked jaw of infants be prevented by cutting the umbilical cord, and applying a cautery, instead of tying with ligature, according to the Indian practice? They compress the end of the cord between the fingers for a minute or two, and apply a hot iron or an ignited coal. Soft cataplasms applied to the umbilicus of infants on symptoms of locked jaw may also be expedient. I believe, however, that the disease depends less on the mode of cutting, or application made to the cut end of the cord, than on subsequent irritation at the *umbilicus* on its spontaneous separation from the abdomen, which usually occurs within four or five days after birth, and is most frequently attended with symptoms of lock-jaw ; and the rise and progress in these cases are strictly analogous to traumatic *tetanus* following wounds in grown subjects. If this be a just view of the case, the inference is, that the greatest attention should be paid to that part ; and in assisting nature by cleanliness, and the most soothing application, to throw off the dead part, without perturbing the frame of the delicate infant.

We may calculate on opium being almost a certain preventive of locked jaw, as it is even efficient, not unfrequently, after the disorder is confirmed, and when the greatest power is required to break the spell. In incipient cases, it will generally remove all symptoms. Why not then employ it as a preventive of *trismus infantum*?—One drop of laudanum given on the second day after birth, increasing it by one drop daily till the child has passed the period of danger, or increased by half a drop per dose, given night and morning, more or less, according to observed effects, we have reason to believe would prevent the approach of the disease. If the child appears livid in the face and the surface of the body, as is often the case, the cord should be allowed to bleed a little, about a spoonful; then pinch the end, and apply a narrow slip of old linen around the stump, just at the extremity; bind the belly loosely, and keep the child dry.

It further seems probable, that one of the efficient causes of lock-jaw in new-born infants lies in the purging plan so commonly pursued. Of this I am fully convinced, since I have found the disease in adults to be constantly aggravated by purging. The plantation Walton Hall, on the coast of Essequebo, lost many infants under the usual treatment, when, about eight years since, a direction-paper, from a supposed connoisseur, was suspended at the entrance of the Lying-in-House, which specified that the navel should be dressed with spirits of turpentine, and that a certain purging mixture should be given daily. I do not remember the composition; but castor-oil was the main ingredient. These directions were followed for several years, during which time most of them were destroyed by locked jaw. This practice was continued until the present manager, Mr Keane, broke the spell; and since that time, I am informed they have been more fortunate.

Opium, especially when conjoined with laurel oil or camphor, is a powerful remedy in locked jaw, whether in new-born infants or adults; but, by the universal practice of purging in this disease, the effects of these potent remedies are counteracted—in general, indeed, totally defeated. And not only so, but a fertile source of irritation to the tender bowels of the infant is thus superadded, directly tending to induce tetanus at a time when the nervous system is already in a state of perturbation from the separation of the *umbilicus*.

The usual dose of castor oil given by the nurses to new-born infants is a teaspoonful, *i. e.* one-eighth of an ounce. Now, according to the table of proportional doses, we observe that it is to a child of *two years of age* that is assigned one-eighth of an ounce. The nurses of Demerara, therefore, give a new-born infant of one or two days old a dose befitting a child of two years; and if the infant proves not to be endowed with powers of resistance equal to those of a young colt, it naturally gets the locked jaw; and then the effects of the neglect of the nurse and mother are ascribed to the smoke and the cold, which, no doubt, contribute to the same effect, but are scarcely sufficient to bring on the disease without more powerful sources of irritation. Hundreds of other causes are also alleged equally valid.

On the occurrence of incipient symptoms of the disease, I have, in several instances, observed much irritation, and a livid appearance of the umbilicus, after the separation of the cord, from neglecting the part, and binding too tight a bandage round the belly; and I have seen it quickly relieved by cleanliness and a warm emollient cataplasm.

Stafford Place, Pimlico.

ART. XI.—*Case of Empyema, or Chronic Pleurisy terminating favourably.* By WILLIAM CLELAND, Esq. Surgeon, Perth.

MISS JESSIE CAMPBELL, seven years of age, slenderly formed, and of light complexion, had an attack of the common remittent fever of children about the 2d of May 1829. The stomach and bowels were much disordered; and the symptoms were for some time very severe.

About the 11th or 12th she began to be annoyed by a short tickling cough, unattended by pain, difficulty of breathing, or any aggravation of the fever. At first it was so slight as scarcely to attract notice. But, seeming to get worse, some common mucilaginous medicine was prescribed, and afforded so much relief, that little more was heard of it for several weeks.

She was free from fever by the 1st of June. Her appetite gradually returned; and in a few days she was able to walk a little about the house and amuse herself. At this time she had neither pain nor cough; but it was observed that she seldom slept unless when lying on her left side, and began to complain of slight difficulty of breathing on going to bed, when she wished her head more raised than formerly. On the 12th of June she had a smart return of fever, occasioned, apparently, by indigestion. It continued severe until the evening of the 15th, when it abated considerably after the operation of an emetic, which had been given in consequence of her having, during the three former days, repeatedly vomited very offensive matters, mixed with undigested aliment.

On the morning of the 17th June I was informed that she had complained during the night of pain in the left breast. On examination there appeared a little fulness of the mamma, with the faintest blush of redness, with slight œdema of the skin, most distinct about an inch and a half below the nipple. There was scarcely any fever, and her breathing was not worse than it had been for the previous fortnight. On the 19th, the left side of the chest appeared somewhat larger and fuller than the right; but the redness and swelling of the skin had not increased.

In the night between the 19th and 20th, she had a very severe rigor; and several more, but less violent, on the night following. The left side of the chest was decidedly enlarged. Her respiration became shorter and more difficult; and her sleep uncomfortable and disturbed. Yet she coughed very little, and complained of no pain, unless where the skin was reddened, and but slightly even there.

On the 23d the left side of the thorax was very much distended. No respiratory murmur was distinguishable in it by the stethoscope, and the heart, pushed from its natural position, was pulsating on the right of the sternum. The pulse had risen to 110, and respiration was becoming still more hurried and distressing. On drawing the fingers along the space between the fifth and sixth ribs, a sort of ring or aperture was felt, such as is perceived on applying the hand to the belly of a child with umbilical hernia. It lay immediately beneath that part of the skin where the redness was most perceptible.

Seeing no chance of affording even temporary relief, except by means of *paracentesis*, I consulted Dr Malcolm on the subject, to whom I was much indebted for assistance and advice in the course of the subsequent treatment.

On the 24th June, 10 o'clock P. M, we found the poor child suffering very much from difficulty of breathing, and looking most anxious and miserable. She was greatly emaciated, and her skin, in many places, (particularly on the arms and legs,) covered with a dark dry scaly roughness, resembling *ichthyosis*. Pulse 130. I passed a lancet into the chest, making an incision of half an inch in length, close upon the upper edge of the sixth rib; and at the place above-mentioned, where the aperture or separation of the muscular fibres was perceptible through the integuments. Immediately a large quantity of a dirty-brown coloured fluid, of the most nauseous fetor, spouted out with violence. Between seven and eight ounces were received into a basin, and as much more, I believe, was lost on the floor. A tent was put in the wound, and a roller round the chest; after which the patient was laid in bed very much relieved.

An hour afterwards, when I called with a medical friend of her father's from Dundee, her pulse had fallen to about 100, and she was breathing pretty freely.

8 o'clock P. M. Had passed an easy day, and taken a little nourishment. The bandages were saturated with most offensive matter; yet, on removing them, the left side of the chest appeared nearly as large as before the operation, and the belly was hard and tumid. The tent being withdrawn, fifteen ounces of

fluid came rapidly away on pressing the abdomen; and the swelling, both of thorax and abdomen, fell considerably after its discharge. It was as fœtid as in the morning, and mixed with thick white flakes. The tent was not replaced, and the roller was applied from the pubes to the axillæ. The breathing was quite relieved; she complained of no pain; her skin was cool and moist; and the pulse 120.

June 25th, 11 o'clock A. M. Had slept nearly all night, and breathed easily. The discharge having soaked through every thing about her, her aunt had removed the roller, and changed her linen at five in the morning, and they were again thoroughly wetted and uncomfortable. The shape of the breast was improved, but there was still enlargement; and the abdomen, although not so tight as formerly, was still tumid. During the dressing not above a teaspoonful of matter could be squeezed away. It was not so dark coloured, but as fœtid as before. Her tongue was clean. Bowels had been twice moved since former visit. Pulse 117.

8 o'clock P. M. Had passed a comfortable day. Had a good appetite, and taken beef-tea, grapes, &c. The bandages were not so very wet as in the morning. A probe was dipt into the wound to separate its lips. Pulse 117.

June 26th, 11 o'clock A. M. Had another good night. The discharge had been most profuse; and the shape both of chest and abdomen was found to be much improved. Had had no cough since the operation. Pulse 105.

8 o'clock P. M. Had slept some hours during the day; and taken beef-tea, arrow-root, &c. Her bowels had been moved three times. She made no complaint of her breathing, but it was hurried. Pulse 110. The roller was dry. Nothing was discharged until the probe was introduced to keep the lips of the wound apart, and pressure made on the belly, when about two ounces of pus came away, thicker and whiter than any hitherto discharged.

June 27th. Had slept well, part of the night upon her back, with her head as low as before her illness. She spent a comfortable day; had a good appetite; and was singing to her grandmother. An ounce and a-half of pus came away at the evening dressing. Pulse 105.

June 28th. Having passed a good night, she was found in the morning asleep upon her right side; her head low; and her breath quite easy. The day following she was allowed some beef-steak, and a little port wine.

June 30th. A small ropy slough being removed from the wound, five ounces of pus came away. Pulse 117.

July 3d. Had passed a bad night; with pain about the chest, and general uneasiness. Towards morning the wound discharged freely, and she became more comfortable. Tongue slightly furred; appetite not so good; pulse 120. Three grains of calomel, with half a grain of opium, were given at bedtime.

July 4th. The medicine operated both on the stomach and bowels, and she was much relieved. I may here mention, that half a grain of opium, with a grain of calomel, was given every night at bedtime for several weeks from that time; and that she took half-a-glass of port wine three times a-day, which quantity was gradually doubled, and continued for many months with the best effects.

July 6th. Had an airing in the forenoon, and returned home with an excellent appetite. The quantity of discharge varying from two to four ounces at each dressing.

The left side of the chest continued gradually to recover its natural size, until, by the middle of the month, it was evidently more contracted than the right.

She now began to suffer considerably at the dressings, in consequence of various sinuses forming in the vicinity of the wound, with tenderness of the breast generally, and suppuration of the cellular membrane in several places. It was discovered that a subcutaneous opening existed between two of the ribs, about two inches above the nipple, by which much of the matter from the chest escaped, and, after diffusing itself under the mamma and integuments, was carried off by the original wound. And, notwithstanding much care in the application of compresses, &c. another sac formed gradually below the wound.

Still, however, her health and looks continued to improve. She was in the open air every fine day; and recovered strength enough to enable her to walk on foot a moderate distance without much fatigue. But her pulse remained frequent; being generally about 100 in the morning, and in the evening 120.

Towards the latter end of July she was not so well. She became uncomfortable and feverish; and her sleep was unrefreshing. This being obviously occasioned by the state of the parts around the wound, the sac was incised freely at its most depending point, (rather more than two inches below the former opening) and she experienced considerable relief in consequence. The original puncture then healed up.

By the 10th of August the new opening was contracting very much; and the discharge of matter being consequently obstructed, she again became more feverish and uneasy. The ribs began again to rise, and the whole side of the thorax to distend in the same manner as at first, although not nearly to the same extent. The recumbent posture also became again uncomfortable; she required more pillows; and lay only on her left side. A bistoury was therefore run upwards from the only wound which was then open, to beyond the scar of the first; at which place, however, the puncture between the ribs had closed, and the whole of the matter from within the chest was now found to be discharged through a sinus communicating with the cavity of the thorax, about two inches above the nipple, as formerly described.

From that time the discharge came away freely of itself between the hours of dressing, and she improved steadily. At the dressings about a drachm of white healthy matter could be squeezed from the sinus; after which a small quantity of matter, thinner, and as fœtid as the first discharge, was forced from within the chest by means of coughing, and this continued to be the case for many months.

She went to Dundee for a few days in the latter end of August, and returned stronger and better, with a pulse not exceeding 100, morning or evening.

As the quantity of discharge diminished, the left side of the thorax became more contracted. Towards the end of September she measured from the centre of the sternum to the spine two inches less on the left side than on the right. Along with this a degree of lateral curvature of the spine had gradually taken place, which, however, did not increase after that time, and since then has considerably lessened, the projection of the points of the scapulæ being now nearly equal. The twist, indeed, never amounted to any very obvious deformity, the contraction being most considerable in the lower parts of the chest.

The pulsation of the heart continues to be felt only on the right of the sternum. The respiratory murmur is distinct over the whole lung, but weaker than on the right side, except at its upper parts, where it sounds strong and healthy.

She went on gradually improving in strength and looks, and during the whole winter never had a cold. A small quantity of fetid matter continued to be forced by coughing from the opening in the chest every day until the latter end of June last, when it disappeared for about a fortnight, but returned for a short time in July. A small sore of the size of a *one-pea* issue, continued open in the beginning of August, when she left Perth; and, as I have since learned, remains still in the same state. She is now in Edinburgh, in good health in every other respect, and fatter than before her illness.

Perth, 2d November 1830.

Art. XII.—*Case of Cæsarean Operation.* By Dr M'Kibbin, Surgeon to the Belfast Lying-in-Hospital. Redacted by

JOHN WALES, Esq. Surgeon, Belfast; and Communicated by WILLIAM CAMPBELL, M. D. Lecturer on Midwifery, &c.

ANN M——, æt. 26, of a healthy aspect, and apparently well formed, was seized with labour-pains for the first time, on the evening of Sunday the 27th September 1829. She was on Monday morning visited by Mr Maclurkan, surgeon, who found on examination the principal part of the left side of the *pelvis* occupied by what appeared to be a large *exostosis*, filling up the hollow of the *sacrum*, and extending from this bone forward to within about a quarter of an inch of the left *ramus* of the *pubis*. The greatest space afforded for the passage of the fœtus (which was found with the hand presenting) was on the right side of the pelvis, *i. e.* between the tumour and right brim; and here the diameter at its widest part was computed at from $1\frac{1}{3}$ to $1\frac{3}{4}$ inch, becoming gradually narrower as it approached the *sacrum* on one side, or *pubis* on the other; the long diameter from pubis to right sacro-iliac *synchondrosis* was calculated at from 3_{3} to 4 inches. Considering delivery difficult, if not impossible, *per vias naturales*, Mr Maclurkan availed himself of the advice of Drs M'Donnell and Thomson, both experienced accoucheurs, who fully concurred with him in opinion that *embryulcia* was impracticable. The patient being in indigent circumstances, was sent into the Hospital at eight o'clock on Tuesday evening, when, on a farther consultation of the officers of the institution with some of the most experienced surgeons and accoucheurs of the town, the Cæsarean section was determined on, and the following were the principal reasons which led to such a decision.

1*st*, The consideration of the dimensions above-mentioned.

2*dly*, Although a mutilated child might by possibility be dragged through such an aperture, the violence which must necessarily be employed, and the protracted exertions, would endanger the life of the woman as much as the Cæsarean operation, and be attended with the certain destruction of the child.

3*dly*, Although the motion of the child had not been felt since eleven o'clock A. M. (Tuesday,) and some meconium had passed, which rendered it probable the child was dead, yet these signs being fallacious, and no direct evidence existing of its death, it was thought advisable to attempt to save it by the Cæsarean section, as the life of the mother did not seem to be placed in greater jeopardy by it than by *embryulcia*, under such unfavourable circumstances.

Accordingly, at a quarter to eleven o'clock P. M., Dr M'Kibbin, surgeon to the Hospital, commenced the operation, assisted by his colleagues, and in presence of several medical gentlemen. The bladder having been previously emptied, the patient was placed on a firm mattress on her back, with her shoulders slightly elevated. An incision of about seven inches in length was made through the skin and cellular substance, (commencing about two inches above the *umbilicus*, and an inch to the left side of it,) in the direction of the *linea alba*. The sheath of the *rectus* being laid bare, and the muscle exposed the whole length of the wound, a small opening was scratched through it at the upper part of the incision, and a probe-pointed bistoury introduced; the fingers being used as a director, the *rectus* was divided the whole length of the external wound, bringing the body of the *uterus* into view, no intestine intervening. Having ascertained the situation of the *placenta*, which was at the *fundus*, an incision of about five inches in length was made into the uterus, (beginning about one-third from the fundus, immediately under the placenta,) which exposed the fœtus enveloped by the membranes; a little gentle pressure on either side of the abdominal parietes ruptured them, and the fœtus was easily removed. The placenta being detached, was also extracted, and the uterus immediately contracted to the usual size after delivery. The hemorrhage was trifling, not more than 6 or 8 ounces of blood being lost during the operation, which the patient bore with great fortitude; and as the uterus subsided into the pelvic cavity, a considerable portion of intestine distended with flatus escaped by the wound, which occasioned some delay, and required a good deal of management to replace and retain *in situ*. The edges of the wound were approximated, and kept in contact by six sutures, assisted by adhesive straps, and a compress and bandage applied over all.

Nausea and some vomiting occurred immediately after the operation; the pulse was 120, and regular. Three grains of solid opium were administered, and directed to be repeated if rejected by vomiting; the effervescing draught was also directed to be given at intervals, while the nausea continued. At twelve o'clock the pulse had subsided to 108, soft, full, and regular; and the patient complained merely of soreness in the situation of the wound. She was closely watched during the night, and an accurate report of symptoms taken every hour. But, without dwelling on particulars, it may be sufficient to observe, that great restlessness prevailed throughout, and, notwithstanding the repetition of the opium pill, and the frequent administration of the effervescing draught, she vomited occasionally the whey given to allay her thirst, which was excessive. She complained at times of an oppressive sense of distension in the epigastrium, which was much relieved by the occasional discharge of flatus by the mouth. Some slight oozing of a sero-sanguineous character took place from the wound; and her pulse fell gradually to 96, full, soft, and regular. This was the state of the pulse at half-past 4 o'clock A. M. (Wednesday;) at five o'clock it had risen to 100; and she now complained of a dragging, or "drawing-in" sensation about the epigastrium, not amounting, however, to acute pain.

9 o'clock A. M. (Wednesday.) Pulse 108, sharp; some trifling sanguineous discharge from the vagina; ten ounces of high-coloured urine drawn off by the catheter. As she complained of some uneasiness in the lower part of the abdomen, and her bowels had not been opened since the operation, fomentations were directed to be applied, and a common enema administered immediately.

11 o'clock A. M. Some low delirium since last visit, which continues. Four injections have been administered without any effect; pulse 120, small and weak; countenance rather sunk; abdomen tender and tense; occasional vomiting; no reaction or heat of surface.

Dr M'Kibbin, on consulting with his colleagues, concluded that the lancet was inadmissible. Fomentations were continued, and the following ordered:

℞ *Ext. Colocynth. Comp. Hydrarg. Submur. aa. gr. iij. M. ft. pilula statim sumenda, et repetatur singulis horis donec alvus responderit.*

2 o'clock P. M. Has been asleep since last report, and the pills have consequently not been given; had one copious evacuation of fluid feces passed involuntarily. Sleep appears to approach coma; eyes partially open, and teeth rather firmly closed. With some difficulty a little fluid was introduced into the mouth, which she made no effort to swallow; it gurgled a short time in her throat, and immediately was followed by a convulsive action of the muscles of the face, which extended to those of the neck, and eventually the extremities were rather violently convulsed for about two minutes. The right side was principally affected. She appears evidently sinking, no rallying effort being evinced. Abdomen still tender and tense; pulse varying from 108 to 130, very irregular and weak. Fomentations continued. From this time she became gradually worse, the convulsions recurring with increase of coma; and she expired at 4 o'clock P. M., seventeen hours after the operation.

Inspection sixteen hours after death.—On removing the sutures considerable adhesion of the cellular membrane was found to have taken place, and the intestines were much distended with gas. There had been no secondary hemorrhage. The uterus had contracted to rather more than the size of a shut hand; its parietes were from an inch and a quarter to an inch and a-half in thickness, and the cut surface was covered with a thin layer of coagulated blood. The outer edges of the incision made into the uterus were considerably separated by the contraction of the circular fibres, so that it gaped very much; the inside of the incision being in contact, and the outer edges asunder thus V; leaving a surface of from 2½ to 3 inches, which during the healing process would discharge its contents into the abdomen more readily than into the uterus. To prevent such an occurrence in future, Dr M'Kibbin thinks, that the introduction of two sutures into the cut uterus would be serviceable in keeping the outer edges of the wound in apposition, and directing the discharges to the natural passages; and that more irritation would be thus removed than would be occasioned by two ligatures cut close, and allowed to remain. Some small specks rather redder than natural were observed on the surface of the intestines, and a slight blush of redness on the peritonæum, covering the uterus, extending about half an inch on either side of the incision.

Head.—The external surface of the *dura mater* healthy; between the *dura mater* and arachnoid, on the right hemisphere, about an ounce and a-half of serum was effused. The vessels on the surface of the right hemisphere were more turgid than those of the left. The rest of the brain and cerebellum was quite natural.

Thorax.—With the exception of some old adhesions of both lungs, at the upper part of the *pleura costalis*, the viscera were healthy.

The *Pelvis* being obtained and divested of soft parts, was found to be well formed, and of the usual dimensions. The whole of its sacral portion, however, (with the exception of the first, and part of the second bone) was enveloped as it were by a large *exostosis*, of a conical form, which extended anteriorly into the *pelvis*, diminishing its cavity considerably, and posteriorly for an inch or more beyond the spinous processes. The *os coccygis* projected from the inferior part of the tumour, and that portion of the latter which extended into the pelvis was smooth in some parts, rough in others, and intersected by two or three superficial fissures; the portion covering the posterior part of the sacrum presented an irregular scabrous surface. The following measurements were taken.

The antero-posterior diameter 4 inches; lateral diameter 5⅝ inches; diagonal diameter 5 inches; long diameter of outlet (from *symphysis* to *coccyx*) 4 inches; transverse diameter (from *ischium* to *ischium*) 4⅛ inches; from the apex of the tumour to the lower part of the symphysis 1¼ inch; from the brim of the pelvis on the right side, (immediately over the *foramen thyroideum*) to the lateral surface of the tumour at its widest part, 1¾ inch, more posteriorly 1⅛ inch; long diameter of the aperture from right sacro-iliac synchondrosis to *symphysis pubis*, 3¾ inches.

From the left brim to the surface of the tumour on that side 1⅛ inch at its widest part, but decreases considerably as it approaches the sacrum.

The fœtus was a female, plump, firm, and healthy-looking, but had evidently been dead some time previous to its removal from the uterus. The usual means were employed to restore animation, without avail. It weighed 5 lb. 4 oz. avoirdupois.

The long diameter of the head (from vertex to chin) 4½ inches.
From ear to ear . . 3 inches.
From one parietal protuberance to the other, 3⅝ inches.
Circumference of the head at its widest part, 13 inches.

The operation occupied about twenty minutes, and was performed with great neatness, dexterity, and coolness.

That the Cæsarean section was in this case imperatively called for will be admitted by every one who reflects on the space afforded for the passage of the fœtus. Let it be remembered, that the dimensions above-mentioned were taken when the pelvis was completely divested of soft parts; and when we consider the space occupied by the latter, it will be evident that the aperture between the right brim and tumour must have been still farther diminished.

The result, which was unsuccessful, adds another to the many fatal cases recorded by British practitioners. The perusal of these cases would be sufficient to deter the rising generation of the profession in these countries from ever attempting a repetition of this operation, even in the most desperate circumstances, were they not influenced by the labours of their continental brethren, who may be said to be generally successful. What, then, is the cause of the fatality so universally attendant, I might say, on hysterotomy, when performed on this side of the channel? Is it in climate or constitution? No. This opinion was many years ago disproved by Dr Hull, who was disposed to consider delay as the great source of the mischief. In this conclusion I entirely agree with him, from perusing most of the successful cases on record, as well as those of an opposite description. Though I admit that our neighbours perform the Cæsarean section in cases where British practitioners would have recourse to *embryulcia* with every prospect of success; on the other hand, it must be confessed, that much valuable time is frequently wasted by the profession here in discussions about an eighth, a quarter, or half an inch of space, while the labour is in the meantime proceeding, and the patient's strength becomes so far exhausted, or inflammation makes such progress before a decision is formed, as to leave little hope of successful termination. The longer the measure is delayed, the life of the fœtus is the more endangered.

I do not say that in most cases the Cæsarean section should be preferred to *embryulcia*. I merely wish to advocate the early adoption of the former in all doubtful cases; that is, where uncertainty prevails as to the practicability of *embryulcia*, and where perhaps no two men agree as to the actual space afforded. In these cases, in which the measurements must be in a great measure conjectural, as no accurate pelvimeter has been yet obtained, I would urge its early adoption, and decided superiority to *embryulcia*. In short, where I was not fully satisfied that a passage existed for a full-grown fœtus of two inches in the short diameter, and three and a-half inches in length, I should be inclined to adopt the Cæsarean operation in preference to *embryulcia*. Cases, I am aware, are recorded, and on high authority too, where *embryulcia* is stated to have been performed with success under still more unfavourable circumstances; but, as no inspection has been adduced to verify these assertions, we may, I think, be permitted to question the accuracy of the stated measurements. I do not find these facts much in favour of *embryulcia*, performed under the circumstances to which I have before alluded, and therefore am not disposed to place unlimited confidence in the statements of individuals, however high their standing may be in the profession. That the destruction of the fœtus may be accomplished effectually, I admit; but the records of medicine attest, that the prolonged and powerful exertions required in contracted or deformed *pelves* to effect *embryulcia* have, in the most experienced hands, produced irreparable mischief, and caused the death of the parent.

I conceive it also essentially necessary, that, in the performance of this as well as of other great operations, the temperature of the apartment should be raised as near the natural standard of the body as possible; this will tend to obviate the evils likely to result from the exposure of a large and sensitive surface to a comparatively cold atmosphere.

The remarks I have made respecting the propriety of early operation are not intended to apply to the case I have just related. Although fifty hours elapsed from the commencement of labour until the operation was had recourse to, yet no blame whatever is to be attached to the medical gentlemen concerned. Mr Maclurkan, with Drs M'Donnell and Thomson, agreed as to the propriety of hysterotomy on Monday morning, and, for the reason stated, recommended her immediate removal to the Hospital. Anodynes were administered in the meantime, with a view of suspending the progress of labour. Her friends, however, were unwilling to consent to her removal at first; and she did not arrive there till 8 o'clock P. M. on Tuesday, when the operation was performed with as little delay as possible. Had she survived, or had impregnation again taken place, the operation must have been repeated; at all events, the disease must have increased in a very short time so far as to interfere with the functions of the bladder and rectum, and caused a painful and lingering death. It is remarkable that she suffered little or no inconvenience from this morbid growth during early life, or the term of utero-gestation. At times she had slight colicky pains; and on questioning her mother as to the probable cause of the exostoses, she admitted that she had received a fall on the back when about six or eight years of age, which occasioned some pain in the sacral region for a short time afterwards.

Belfast, *November* 8, 1830.

ART. XIII.—*Two Cases of Enlargement of the middle or third Lobe of the Prostate Gland, successfully treated by Perforation and Puncture; with Remarks.* By R. A. STAFFORD, Esq., Member of the Royal College of Surgeons in London, and formerly House Surgeon to St Bartholomew's Hospital.

ONE of the most common diseases of the prostate is enlargement of its middle or third lobe, so ably described by Sir Everard Home, and which gives rise either to partial or total retention of urine. As our remedies for the relief of this distressing disease are very limited, the two following cases, in one of which this portion of the gland was perforated, and in the other punctured with success by the lancetted stilette or armed catheter, may not perhaps be considered uninteresting.

Case 1.—A gentleman, about 50 years of age, and of a spare habit of body, applied to me with an impermeable stricture of the urethra, and gave the following history of its formation and progress. About thirty years since, he contracted a gonorrhœa, which, according to the practice prevalent at that time, was treated by powerful astringent injections. The gonorrhœa ceased; but in a few months afterwards the stream of urine became considerably diminished in size, in consequence of which he consulted the surgeon of his regiment, (for he was at that time in the army,) who recommended the use of bougies. These he employed, gradually increasing their size, for some months; and when it was considered that the contraction was sufficiently dilated he discontinued their use. The stricture, however, in a year or two returned, and he again had recourse to bougies. A recurrence of the disease was the result; and it now became so unmanageable that he was obliged to quit the army. At this period very small bougies could be occasionally passed, but they caused great irritation; and if they were discontinued any length of time the part immediately closed up. Some time after this, lunar caustic was applied with the view of destroying the diseased portion of the urethra; but instead of producing the desired effect, it rather increased the malady. The disease being afterwards neglected, the stricture became impermeable; and when I first saw the patient, it had been in that state for two years. Upon examination, I found the obstruction was situate about one-fourth of an inch anterior to the bulb; that it was of an indurated character; and that the prostate gland was slightly enlarged. The urine flowed so scantily that it usually took from two to four hours to empty the bladder, and that in a manner most painfully distressing. He was obliged to elongate the penis to its utmost, to strain with so great a force that the perspiration frequently stood upon the forehead in large drops, and to walk to and fro in this state, writhing in the severest agony, before he could expel a drop. In this manner he could collect, in about five or ten minutes, a small spoonful of urine, and by this method alone could he evacuate the contents of his bladder. During the period of his complaint, he had occasional attacks of retention of urine; and at one time there was an abscess formed in the perineum; as, however, no urine made its escape, it seems doubtful whether this was connected with the urethra.

On July 30th, I began to divide the stricture with the single lancetted stilette, and thinking it probable, from the history of the case, that a considerable extent of the urethra was indurated and diseased, I proceeded with the greatest caution; because if the contraction is of great length, and, as it not unfrequently happens in such cases, tortuous, too great boldness in the use of the instrument would incur a danger of making a false passage, or giving rise to inflammation and tumefaction sufficient to cause complete retention of urine, I therefore never divided at any one time more than the sixth or eighth of an inch. The diseased portion of the urethra was about two inches in length, and its structure varied considerably. Sometimes it was extremely hard, so much so as almost to resist the entrance of the lancet into it; sometimes a gritty substance was penetrated; and sometimes the part was of a more spongy and vascular nature. In about four months the stricture was by this gradual manner divided, and no unfavourable symptom during the whole period occurred. The patient took his usual exercise, and lived on animal food, but he was not allowed to drink wine or fermented liquors. The operation of cutting was never attended with much pain, nor was there ever any loss of blood amounting to more than a dessert spoonful, and this occurred only once. The hardened structure appeared to become absorbed, since each portion after its division remained pervious.

After having permeated so great an extent of the urethra, the patient was attacked by retention of urine, which could not, however, be called *complete*, as he was able to void, though with great straining and extreme pain, about a teaspoonful of urine during the day. The bladder now became considerably distended; the urine was extremely fœtid; and at the bottom of the vessel which contained it, there was deposited a muco-purulent sediment tinged with blood. Having completely cleared the urethra down to the prostate gland, I examined *per anum* the state of this organ, and found it to be rather more enlarged than on the first examination, and that on pressure with the finger it was extremely tender. The pulse at this time was hard and at 90; the tongue very dry and furred; the skin hot; and the countenance full of distress and anxiety.

He was ordered, (November 25 and 26.) to lose blood from the perineum by leeches and cupping, to make use of the warm bath, to foment, to take purgatives and opiates, and to use the latter in injections and suppositories. All these means failed to produce any effect, and the bladder was becoming so distended that there was reason to fear that it might burst, or urinary coma might come on, unless it was punctured; or unless the operation which shall presently be described was performed. At this crisis, therefore, I requested to have another opinion, and my friend, Mr Lawrence, surgeon to St Bartholomew's Hospital, was called. The catheter could at this time be passed to the length of 8½ inches; and on examining its point *per anum*, it appeared to have entered into the anterior part, or mouth of the prostate, and there to be obstructed. In this state of the case Mr L. concurred with me in thinking it would be best to cut on through the obstruction, which appeared to be an enlargement of the third lobe of the prostate, with the lancetted stilette, in as near a line as possible in the natural channel into the bladder. Having introduced the instrument, I thrust forth the lancet at its point, and cautiously perforated the obstruction, until I advanced nearly two inches farther. The stilette now was eleven inches, but no urine flowed. It was therefore withdrawn, and a catheter was passed to the same point, but it would not go farther, until upon examination with the finger *per anum*, in order to discover its exact situation, it slipped into the bladder, and upwards of three pints of fœtid urine were evacuated. In performing the operation the handle of the instrument was depressed as much as possible; and when the lancet which had made the cut had receded into its sheath, the blunt point was thrust in with the greatest caution until it had reached the extremity of the incision. The perforation was accomplished by protruding the lancet three times. The operation produced but little pain, and the bleeding was so trifling, that it could only be perceived by examining the point of the instrument. A No. 9 silver catheter was left in the bladder and secured, and the patient remained in bed. An opiate draught was administered. In the evening the pulse was reduced to 80—the skin had become moist, and the countenance had in a great measure lost the expression of anxiety.

November 27, The urine had flowed freely through the catheter, and my patient had slept about two hours during the night. The pulse 75, and soft; skin moist; tongue less furred, and not so dry; and the countenance had recovered its usual composure. An aperient was ordered, which having produced two motions, the opiate was repeated in the evening.

28th, The urine still flowed freely through the catheter, and occasionally on one side of it. Pulse 80, and soft; skin cool; tongue becoming clean and moist. He had slept during the night for four hours. The catheter was still kept in the bladder, and he took an opiate at bed-time.

The patient from this time went on favourably; and as the catheter began to cause great uneasiness, I found it necessary, one week after the operation, to withdraw it, which I did not do before, because I thought it better to allow the perforated prostate to recover itself as much as possible before another instrument was passed, or the urine was allowed to flow through it. I therefore introduced an elastic gum catheter, and did not even permit the patient to attempt to make water. It passed through the whole course of the urethra without meeting with the least impediment, and the parts at the prostate appeared to be more open than the rest of the canal. The catheter was left in the bladder for another week, when it was taken out altogether. The patient, however, at first was incapable of making water, so that it was necessary to draw it off three or four times a-day, which he did himself without the least difficulty, and what is singular, and proves that the obstruction was chiefly at the neck of the bladder, is, that he could eject the urine by the action of the *detrusor* muscles to a considerable distance through the catheter.

In about three weeks from the operation he began to make water of his own accord, and it was not always necessary to draw it off; and in less than six he could completely empty it of its contents. He now felt but little pain either in the bladder or the urethra; and as he got an attack of jaundice, and began to suffer in his general health from confinement and a London atmosphere, I advised him to return into the country, which he did on January 15, 1830, and accomplished a journey of 350 miles in a carriage in less than five days. I have since heard from him several times, and he has informed me that he was quite recovered. The passage through the stricture and prostate gland remained freely open; and he could void his urine as well as ever he did in his life.

There are two points of interest in this case. *1st*, It shows that an impermeable stricture more than two inches in length may be divided without danger by the lancetted stillette; and, *2dly*, That an enlargement of the third or middle lobe of the prostate gland may likewise be perforated with perfect safety. It may be observed in this case, that the prostate itself was not much enlarged, but that it was chiefly the third lobe. This fact is proved by the previous symptoms, as well as from my being enabled to pass the catheter 8½ inches down the urethra, and there meeting with an obstacle. That this impediment to the complete introduction of this instrument was an enlargement of the middle lobe, is also furthermore proved, by the necessity there was of perforating at least two inches more, making the distance from the orifice of the urethra altogether eleven inches before the bladder was entered. From this circumstance, it would appear that this portion of the gland must have been more than usually increased in size, arising most probably from temporary inflammation. The subsequent symptoms tend to show, that the obstruction was chiefly at the neck of the bladder; for although he could not void the urine of his own accord, he could eject it through the catheter; but when the swelling of the part subsided, he recovered this power, and could make water naturally.

In the case before us, it will be seen that there was neither pain nor hemorrhage from the operation, and that immediately after it the frequency of the pulse subsided, the skin became moist, the tongue clean, the anxiety of the countenance vanished, and the pain abated. The swelling of the prostate gland subsided; the perforation through the third lobe, and the diseased portion of the urethra, remained freely open, and became quite healed, conveying to the finger, when a catheter was passed, a sensation as if it were covered by a membrane analogous to that lining the healthy urethra. The bladder gradually regained its power, and the patient in a few weeks was restored to perfect health.

Case 2.—A gentleman, æt. 49, naturally very thin, but apparently rendered more so from disease, placed himself under my care with the following symptoms. For many years past (nearly thirty) he had experienced great difficulty in making water, and the stream of urine had always been diminished in volume. Latterly, within the last five or six years, these symptoms had gradually increased; and at the present time the water passes from him almost *guttatim*. During the time of making it also he is under the necessity of straining; but if he exerts himself too violently, it gives him great pain, and the neck of the bladder appears to close-up; and thus the escape of the urine is rather retarded than facilitated. His urine is fetid and muddy, and of a dark colour, and the contents of the bladder are not completely evacuated. The state of the urethra is as follows. The natural orifice is completely closed, but there is an opening connected with the canal immediately beneath it. About half an inch anterior to the bulb there is a stricture totally impermeable to a bougie; and anterior to this there is a false passage which had been made two years before by caustic, and which at the time bled so profusely, that the hemorrhage did not cease for more than four days. When a bougie is now passed, it is with the greatest difficulty this deviation from the natural canal can be avoided; and if its point should chance to enter it, blood instantly gushes forth so largely from the urethra, as to give it the appearance of an artery being wounded. The body of the prostate gland is slightly enlarged, but not more so than is common at the period of life at which the patient is arrived.

After enlarging the orifice to its natural size, I gradually divided the diseased portion of the urethra, which extended about an inch and a-half; and though in this case, as in the former, I proceeded with the greatest caution, for the reasons I have already mentioned, the operation was accomplished in four or five weeks. I found when I arrived at the prostate, that the instrument was still obstructed, and would not enter the bladder. The patient could make water in a larger stream; but he had still the same difficulty in expelling it, and the bladder never emptied itself. For these reasons, and as the catheter could be passed rather more than eight inches, and its point could be felt to have entered the prostatic portion of the urethra, I concluded this obstruction could be no other than an enlargement of the third or middle lobe; I therefore, as the symptoms were not very urgent, cautiously made three or four punctures in it. The patient felt some pain, and expressed himself as if something had been incised at the neck of the bladder. No bleeding ensued; and, indeed, so little did he suffer from it, that he felt no greater uneasiness than before the operation. In three days more I made the same number of punctures, which gave rise to exactly the same sensations at the time, but nothing more was felt. After the same interval of time, I again punctured the part, and on my fourth visit, my patient expressed himself considerably relieved. He could make water better, and he thought the bladder had gained more power. To ascertain the state of the urethra, I now introduced a steel sound, which passed easily into the bladder. On drawing off the urine afterwards, I discovered that the bladder could expel only half its contents. A catheter, therefore, was introduced night and morning, gradually increasing its size, until this organ completely regained its power. In about six weeks or two months the urinary organs of this patient were restored to their healthy function.

In this case, it appears that the middle or third lobe of the prostate gland was so enlarged as to form a valvular obstruction at the neck of the bladder, and that, from the repeated punctures in its substance, the swelling of the part went down. It is impossible to say by what process this was effected; nevertheless, the fact opens to our view a method of treating a disease for which before but little could be done. Experience alone can show its ultimate utility; but from mere reasoning, the puncturing of an enlarged and hardened part, with the view of reducing it, appears rational, and facts even, as far as they go, lead us to such a conclusion. The prostate gland is not a very sensitive part, and it does not appear to suffer much injury from a wound of the middle or third lobe.

In the Hunterian Collection, in the Museum of the College of Surgeons of London, there are three preparations in which this portion of the gland has been wounded by the forcible introduction of a catheter into the bladder, without any bad consequences having ensued. In the first, this instrument has perforated the middle lobe, and through the opening thus made, the urine was drawn off for five years afterwards. In the second, the third lobe is completely severed, and the divided surfaces are covered by a membrane analogous to that which lines that portion of the urethra which passes through the prostate; and in the third, the middle lobe, which was not so much enlarged as in the other cases, is also torn, without any apparent inconvenience having been sustained. It unfortunately happens that no particular history of these cases has been recorded.

PART II.

CRITICAL ANALYSIS.

Art. I.—1.—*Minutes of Evidence taken before a Commission appointed to investigate the origin of the late Yellow Fever Epidemic at Gibraltar.*—2. *Answer to the Discourse of M. Audouard against the Report made to the Academy,* 15*th May* 1827, *on the Documents concerning Yellow Fever.* By N. Chervin, Docteur, &c. Paris, 1827.—3. *Second Answer on the nullity of the pretended Facts of Contagion observed at Barcelona in* 1821. By N. Chervin. Paris, 1827.—4. *Report to the Royal Academy of Medicine at the Meetings of* 15*th May and* 19*th June* 1827, *in the name of the Commission charged with examining the Documents of M. Chervin regarding Yellow Fever.* Paris, 1828.—5. *Critical Examination of the pretended Proofs of the Contagion of Yellow Fever observed in Spain.* By N. Chervin. Paris, 1828.—6. *Examination of the New Opinions of Dr Lassis regarding Yellow Fever, &c.* By N. Chervin. Paris, 1829.—7. *On the Opinion of the American Physicians on the Contagion or Non-Contagion of Yellow Fever.* By N. Chervin. Paris, 1289.—8. *Letter to M. Monfalcon on the Yellow Fever of Gibraltar* 1828. By N. Chervin. Paris, 1830.

THERE is not a better example of the difficulty of determining matters of fact, and arriving at precise and legitimate conclusions, than is furnished by the questions regarding the origin and characters of yellow fever. While one party maintains that it is in all instances of foreign growth; that it is imported in the persons or clothing of human beings; and that its appearance may be in all cases prevented by prohibiting intercourse and enforcing rigorously the quarantine system of separation and exclusion;—another party contends with no less energy, that it is in all instances of local or domestic origin; that it cannot be conveyed from one to another part of the world, or communicated from one person to another; and that, consequently, every measure which has for its object mere prohibition of intercourse is futile and inefficient. Any one acquainted with the process of induction employed in other sciences would imagine, that parties maintaining opinions so diametrically opposite must necessarily have deduced their conclusions from different facts, or at least different kinds of facts. But such is the peculiar character of medicine, and of what has been denominated medical logic, that in almost every instance the facts are the same; and it is only in the mode of application, and in the conclusions deduced, that the observer recognizes a difference.

It may be imagined that in such a controversy the opposite partizans entertain no friendly feelings toward each other; and every impartial inquirer observes that they impute to each other the worst motives, and that each looks on his antagonist, not as a liberal inquirer, anxious to discover true facts, and to draw legitimate conclusions, but as a sophistical uncandid reasoner, ready to distort facts to suit his own views, and not unfrequently to suppress those which are hostile to them. To the same spirit must be ascribed much of that strong, intemperate, and not very decorous language in which we have seen several of these angry disputants indulge. Though we do not adopt the rule, that in all cases the violence of language is in the inverse ratio of the strength of arguments, we are convinced that the arguments are infinitely more forcible, and much more likely to make an impression where the declamatory tone is entirely avoided, and where the parties confine themselves to a simple detail of facts, and a cautious and temperate statement of arguments and deductions. In perusing the accounts of the same yellow fever epidemics by different authors, it is obvious that a large proportion of what the writers imagine to be indisputable facts and conclusive reasoning are neither more nor less than a series of assertions, not unfrequently of a very declamatory character, and gratuitous assumptions without any foundation in fact. Thus, while in almost all the examples of yellow fever epidemics occurring either in the United States or in sea-ports of Europe, one party trace the first appearance of the disease to the arrival of some vessel from Havannah, or some of the islands of the West Indian Archipelago, the other strenuously deny any connection save that of coincidence, and even this often they will not admit; while they labour to show that the physical and local peculiarities of the place are adequate to the production of the disease. It is nevertheless a certain fact, that the West India islands are not in all seasons productive of yellow fever; and when they are, it is not easy to see by what means the disease, originating as it is represented to do, could be transferred to Europe or America. The determination of this point is, under the present circumstances, one of extreme difficulty. But if it could be clearly established, that, in the case of a European epidemic, no vessel of sickly or suspicious character had touched her shores, it might be regarded as a strong proof that the disease was at least not of foreign origin. To ascertain whether it arose from the operation of local causes would be a task still more difficult, and in all probability is not to be decided by positive evidence. We propose at present to inquire strictly how far either questions can be considered to be determined by the phenomena of some of the recent European, and especially the last Gibraltar, epidemic.

The question of the origin of yellow fever and the mode of its propagation, involves another preliminary one on which there has been much discussion, viz. what is the nosological character of the disease? and when this is determined, it may be said, that in one sense that regarding its origin is also decided.

It is agreed that a fever of peculiarly fatal character, rather rapid in progress, and attended towards its termination with yellowness of the skin and vomiting, and occasionally purging of dark-coloured coffee-ground-like matter, has been observed to prevail epidemically for certain seasons in several of the West India Islands, and on the coast towns of the American continent near the tropics. It is certain, at all events, that such a fever has often been described by various observers, medical or general, under the name of *New Distemper*, *Kendal's Fever*, *Mal de Siam*, *Fievre Matelotte*, *Vomito Prieto*, *Chapetonada*, and, more recently, from the appearance of the surface, *Yellow Fever*. On the origin of this fever no very distinct opinions had been entertained, save that it was peculiar to the countries in which it appeared, and was probably connected with the soil or the climate of these regions. Several authors, and, among others, Ulloa and Dr John Hunter, evidently regarded it as proceeding from the same general causes as the malignant remitting fevers, and differing chiefly in degree, and in the more epidemic character of certain seasons. The idea of contagion appears never to have been entertained, and certainly was not hitherto associated with the malady. In the year 1792, however, a circumstance occurred which gave a new view of the origin of this disease.

With the view of putting an effectual stop to the slave trade, and introducing civilized habits among the negroes of the slave coast of Africa, it was proposed to found a European colony at Bulam or Bulama, an island about 120 miles in circumference, near the coast at the mouth of the Rio Grande. An association formed for the purpose, accordingly chartered two vessels, the Calypso of 298 tons, and the Hankey of 260, with a sloop of 34 tons, which sailed for Bulam on the 12th of April 1792, having on board a healthy crew and 149 colonists in the former, 120 in the latter, and 6 in the sloop. The vessels having separated in a storm, the Calypso arrived on the 25th May at Bulam, about twelve days before the Hankey. The rainy season commenced on the 4th of June; and soon after fever appears to have attacked several persons on board the Calypso, and afterwards in the Hankey. From various circumstances, the colonists became dispirited, and came to the resolution of abandoning the colony; and, accordingly, 147 persons sailed from Bulam in the Calypso on the 19th July for Sierra Leone, and thence for London, where they arrived in the middle of November, after the loss of 40 persons by the sickness.

In the meantime, the remaining colonists, consisting of 48 men, 13 women, and 25 children, undertook to establish themselves on the island. From various causes, but especially the brackish nature of the water of the island, and the savage disposition of the natives of the coast who occasionally visited the island, it was found impossible to erect any permanent means of accommodation; and for nine months, during which the Hankey lay off Bulam, the settlers were under the necessity of living on board the vessel. In this situation, as the severity of the rainy season was great, these colonists, with the view of sheltering themselves at once from the moisture and the heat, which at this time was excessive, raised the sides of the ship several feet by wooden planks, and covered the whole by a deal roof. In this habitation about 86 persons, men, women, and children, and not upwards of 200, as represented by Dr Chisholm, were confined in a moist sultry atmosphere for several months. The sickness meanwhile proceeded, until 48 persons of the 86 were destroyed, of whom 23 are stated to have died of fever. In this state the Hankey sailed from Bulam on the 23d November, touched at Bissao on the 26th, and finally sailed for England on the 3d December. The following night, however, she received such injury on a sand bank as obliged her to return to Bissao to refit, and finally sailed from this port on the 21st December. On the 26th, she arrived at St Iago, in the bay of St Francis; and on the 4th January, anchored at Port Praya, after losing by deaths, since her departure from Bulam, 8 of the crew, and 5 men, 3 women, and 2 children of the colonists, or 18 persons altogether. These losses had so much reduced the number of effective hands, that the Hankey was now unable to proceed to England; and, having remained about eight days at Port Praya, the arrival of the Scorpion and Charon, two ships of war, supplied them with some necessary stores, and two men from each ship, on the 2d January, for the purpose of proceeding to the West Indies. One of the men sent from the Charon died afterwards on the 4th February. The Hankey in the meantime sailed, and, after a passage of 19 days, arrived at Barbadoes, and finally anchored in St George's Bay at Grenada on the 19th February, in a state of perfect health.

Soon after this time fever appeared, first sporadically, and then more generally, among the crews of vessels lying in the Carenage of St George's Bay at Grenada; and, about the middle of April, sickliness appeared among the inhabitants of the shore, chiefly among the soldiers; afterwards, in June, among the negroes of the estates in the neighbourhood of the town; and eventually spread, not only over the whole of the island of Grenada, but various others of the windward groupe. The particulars of this epidemic infliction, which are given in various publications of the day, it is unnecessary to specify further. But, from the fact that it began to appear shortly after the arrival of the Hankey, Dr Chisholm forthwith inferred, that it was a new pestilential malady imported from the island of Bulam, altogether distinct from the proper yellow fever of the Antilles, and remarkable not only for its high mortality, but for its contagious character. The justice of these views regarding this disease it would be difficult to refute directly. While Dr Chisholm maintained that the epidemic disease which prevailed in the island of Grenada in 1793 and 1794 was altogether new and unknown in these islands previous to that period, there was a possibility of his being in the right, when he represented it as endowed with peculiar contagious properties, and as altogether different from the yellow fever which had been observed in these regions during many previous seasons. There were, however, three circumstances in this matter which rendered the conclusions of Dr Chisholm very questionable, if not altogether erroneous. It was, in the *first* place, a well-established fact, that the disease represented to have been imported by the Hankey was by no means a new disease, but in all respects the same as the yellow fever of the Antilles. *Secondly*, it is a historical fact, that this was not the first appearance of the disease in question; that there had been many previous epidemics recorded by different authors in the West India islands; that scarcely a single season was destitute of sporadic cases among the Europeans recently arrived; and that it differed from former epidemics only in being more general, and, on the whole, more fatal. And, *thirdly*, the connection between the fever which proved so fatal to the Bulam settlers, and that which afterwards prevailed so extensively in Grenada, was purely accidental. The Hankey was in a state of perfect health on her arrival in the West Indies; and, assuming that the Hankey could have imported disease from that island, it could not have been yellow fever, even according to the hypothesis of Dr Chisholm, since the disease which attacked the settlers there was simply the remittent of the West African coast, which is well known still to prevail at Sierra Leone, Fernando Po, and along the whole coast, and up the banks of the rivers.

In the *first* place, it is a historical fact, that genuine yellow fever has existed at Vera Cruz, Havannah and the whole east coast of America, long before the appearance of the Grenada epidemic of 1793. It was observed at Carthagena and Terra Firma in 1729, at Guayaquil in Peru in 1740; and it appears to have been the disease which committed such frightful ravages in the squadron of Admiral Hosier before the Bastimentos of Porto-Bello in 1726, and in that of Vernon before Carthagena in 1741, and at Porto-Bello in 1742. It was observed even previous to this period in 1647 at Barbadoes by Ligon, and again, according to Hughes, in 1695. In North America, it appears to have been epidemic at Boston in 1693, and at Philadelphia and Charlestown two years after; at New York in 1702; at Virginia in 1744; at Charlestown again in 1732, 1739, 1745, and 1768, and at Philadelphia again in 1751 and 1762.

In the *second* place, if in any of these or similar situations yellow fever is occasionally not observed for a series of years, it is not so much that its productive causes do not exist, or are not in operation, as that there are no subjects on which this operation can be exerted. After any great epidemic visitation, most, if not all, the susceptible persons are cut off, and, until a new supply accumulates, the epidemic character cannot be assumed, and the sporadic only is observed. In some places even which are either thinly or not at all inhabited, yellow fever cannot by possibility exist, yet occasionally appears when these places are visited by vessels or troops. Thus Honduras and the Mosquito shore are so thinly peopled that the disease was not known till the year 1780, when the expedition under General Dalling proceeded from Jamaica against the Spanish dominions. Almost immediately after debarkation in the San Juan river on the Mosquito shore, the sailors were attacked on board the ships, and the soldiers before the castle of San Juan with ague, remittent and yellow fever; and the mortality was in a few weeks so enormous that the purpose of the expedition was altogether defeated. Yet, notwithstanding this lesson, which has been recorded by Moseley and others, the present generation lately witnessed an attempt to colonize this part of America; and the undertaking terminated, as might have been anticipated, in the ruin of the colonists.

The third point has been already considered in our account of the voyage of the Hankey.

Slender, however, and unsubstantial as this foundation was, Dr Chisholm, in defiance of the historical facts of the case, reared on it a hypothesis, that a new and unknown malignant pestilential disease was imported into the West Indies; and that, to guard against its propagation, the most rigorous measures of separation and seclusion were at once requisite and effectual. This hypothesis would probably have quietly sunk into merited oblivion, had it not been revived by the efforts of a second champion, Mr now Sir William Pym, who partly, on occasion of its appearing at Cadiz in 1800, at Carthagena in 1804 and 1811, and at Gibraltar in 1810 and 1813, partly in consequence of the publication of Dr Bancroft in 1811, described as Bulam fever, all the varieties of yellow fever epidemics which had appeared in these several years and situations.

It is singular that in this mode of representation Sir William Pym has not been able to recognize the fallacy and inconclusiveness of his reasoning even on his own hypothesis. No fact is more certain or better established, than that the Bulam fever is merely the remittent in various degrees of intensity of that coast, becoming occasionally more severe according to the season, and more epidemic as it has subjects on which to operate. If, therefore, his inference be correct, that this and the yellow fever are identical, it must follow that the latter is not contagious, but, like the former, originates in local causes and climatic peculiarities. As to the original commencement of the Bulam fever by the crowded state of the crews and colonists of the Hankey, however favourable these circumstances might have been for the production of typhoid or contagious fever, they could have had no influence in the production of the genuine Bulam disease. In these circumstances, therefore, the question of the disease alleged to have been imported by the Hankey, is divided between plague and contagious fever; and while it is certain that it was not the former disease, it is equally certain that the temperature was too high to favour the formation of the latter, which is principally a native of cold climates and seasons.

In short, the whole doctrine of the Bulam fever in the West Indies, and of its contagious properties, originated in the simple assumption, that the original fever of Bulam is contagious, and that this was imported by the Hankey into Grenada and the West India islands; and as this doctrine, when divested of adventitious and extraneous circumstances, loses altogether the essential circumstance of filiation between the two events, no impartial reasoner would hesitate in announcing them as altogether unconnected.

The same principles have been applied to the appearance of yellow fever of late years at Barcelona, Carthagena, Cadiz, and other places in the south of Spain, and, above all, the recent epidemic visitation at Gibraltar; and strenuous attempts have been made to trace the first appearance of these maladies to the arrival of sickly vessels from the Havannah or West Indies, in those ports, in the vicinity of which the first traces of the disease were developed.

The doctrine of contagious propagation once assumed, appears to have been generally maintained, not so much on the conclusive nature of the facts by which it was believed to be proved, as by the bias and preconception with which all these facts were observed and recorded. While we do not positively assert that it is false that the yellow fever is contagious, yet examination of the evidence on which this doctrine has been maintained leads to the inference, that it is altogether defective and inconclusive. Sir James Fellowes, for example, who is infinitely the most moderate, temperate, and philosophical of the writers who have supported this side of the question, admits throughout its importation by West India vessels, and its propagation by sickly persons to their attendants and others.* Sir Gilbert Blane, on the other hand, one of the most zealous of those who have supported the side of contagion, and who has taken particular pains to give his statements and arguments an appearance of logical force and precision, is obliged, nevertheless, to adduce evidence, which, if strictly examined, turns out to be almost invariably mere matter of opinion, or the authority of those who were of the same mode of thinking. Thus for his three principal arguments in favour of the contagious nature of yellow fever, viz. its first appearance in sea-port towns and not in inland situations; its being capable of being traced to the arrival of one or more vessels from parts in which the disease was prevalent; and its being at first confined to those who had direct communication with the shipping,—the authorities are the writings of Sir James MacGrigor, Sir James Fellowes, Mr Pym, Sir Joseph Gilpin, Dr Stewart of Grenada, Dr Gordon of St Croix, Dr Arejula, and the French commissioners, all of whom, with the exception of the first, have distinguished themselves as the decided advocates of contagion.

In a manner still more vague and general do the French commissioners Bally, Francois, and Pariset reason, when speaking of the contagious characters of the Catalonian epidemic. "Of the eminently contagious character of this disease," say they, "we have had superabundant proofs. The most enlightened physicians of Barcelona, Tortosa, Palma, Mahon, entertain no other belief. The contagious character of the other yellow fever epidemics observed in Spain is almost unanimously maintained by the physicians of Cadiz, Xeres, Lebrija, San Lucar, Port Santa-Maria, Chiclana, Carthagena, and Alicant. Thanks to the cares of those gentlemen who have acted as French consuls in Spain, we have received on this point the most direct and numerous testimonies. In a single city, for instance, at

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* "The contagion was, however, conveyed by emigrants from Cadiz to the neighbouring towns; the communication had been cut off too late, the disorder spread rapidly to them all; and the attempts to check its progress afterwards were ineffectual." —Reports of the Pestilential Disorder of Andalusia, &c. p. 47.

Cadiz, twenty-five physicians have subscribed a declaration which has been transmitted to us, and which confirms every thing that we have hitherto advanced. One of them, M. Gonzalez, in a separate declaration, goes so far as to affirm that the yellow fever is more contagious than the plague. The conviction of this property of yellow fever has become so strong and so general in Spain, that it is on this conviction that the enactment of the sanitary laws has been founded."—*Histoire Medicale*, p. 131.

It is impossible to doubt that in such a statement as this there is nothing which bears any resemblance to evidence or reasoning. The opinions of these physicians, however estimable, are nothing but the opinions of men; and we all know how many erroneous and gratuitous notions have been adopted and currently propagated on the ground of mere opinion, totally unsupported by facts. The mode of establishing a point of this description is not by collecting the opinions and suffrages of different physicians, and deciding according to the number or the weight of the names, but by observing facts which cannot be explained in either way, and which, if accurately observed and faithfully reported, must decide either on the affirmative or the negative side of contagion.

With regard to the preliminary question of the identity of the disease with that of yellow fever, we believe that can by no means be doubted. In the European epidemics, indeed, the disease is generally longer in duration and slower in progress, and perhaps, on the whole, rather milder. But in all the essential characters and phenomena which constitute identity of nature it is the same. In both we recognize the same affection of the head and gastro-enteric organs; in both we observe the approach of yellowness, at least in the majority of instances, as the disease proceeds; and in both there is ejected from the mucous surfaces, especially those of the alimentary canal, the same kind of dark-coloured bloody matter. In both, also, from the recent testimony of many observers, but especially of Jackson, O'Halloran, Proudfoot, Smith, Amiel, Chervin, and others, the morbid appearances exhibited by inspection are as nearly similar as can be expected in a disease affecting such a number of organs simultaneously. On this point, however, we refer to the minute and elaborate parallel of symptoms and appearances given in the Medical History of the Yellow Fever of Catalonia by MM. Bally, Francois, and Pariset, which shows that no doubt can exist as to the identity of the disease. *

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* Histoire Medicale de la Fievre Jaune observée en Espagne, et particulièrement en Catalogne, dans l'année 1821. Par Bally, Francois, et Pariset. Paris, 1823, 3tième partie, p. 513.

Supposing it then to be established, that the yellow fever of Europe and the south coast of Spain is in all essential circumstances the same disease as that of the Carribbean islands and the American coast, we proceed to inquire what are the proofs of its contagious character and importable property.

The first condition requisite to demonstrate the contagious character of yellow fever, is to prove that it is directly transmitted from the persons, clothing, or bedding of those sickly, or those who have been sickly, to those in health. In order, however, that this test be free from all fallacy, it is indispensably requisite that the persons to whom the disease has been supposed to be communicable do not reside in the same situation or locality in which those by whom it is believed to be communicated are. Thus, it is altogether nugatory to adduce on this point, the examples of those persons who are living on the wharves, and in the neighbourhood of coasts, because these persons are exactly in the same locality in which the diseased are, and, it may be safely argued, are exposed to the same local causes, whatever these may be. It is on this account that the cases of the washerwoman, Mary Parody, the waterman, Andrea Andre, and the other persons who sickened at the commencement of the recent Gibraltar epidemic, are of no consequence whatever in determining the contagious characters of the malady, since all these persons resided in situations in which local causes might have as readily operated as exposure to alleged contagion. The first appearance of the disease, also, in the district, No. 24, shows, as mentioned in the proceedings of the Commission, and in the observations of Dr Smith, and the answers of Mr Amiel, that it is equally impossible to say, whether the subjects of these cases derived their disease from intercourse with the crew of any of the vessels or not.

In all the instances of European yellow fever epidemics, it has been the custom to endeavour to trace the first appearance of the disease to the arrival of some vessel or vessels from the West Indies; and in the case of the late Gibraltar disease, a Swedish ship, named the Dygden, was the only one on which this suspicion could be plausibly fixed. This vessel had arrived from the Havannah in the bay on the 27th June, after a passage of 47 days, and, according to the usual practice, she was condemned to a further non-intercourse period of 40 days by the quarantine laws. To connect the arrival of this vessel with the appearance of the fever, it was alleged, that a cigar-dealer, named Felix Fani, had ventured on board shortly after her arrival; but this has been subsequently denied in the strongest and most solemn manner by his widow. It appears further, that this man died not with symptoms of black vomit, but with those indicating an intestinal inflammation, probably strangulated hernia. His children also do not appear to have died of fever, but of some disorder of the intestinal canal, which the Spanish physician who attended them attributed to indigestion from eating green figs. Independent, however, of the interruption of causation in this part of the chain of connection, it appears in evidence, which is quite as much entitled to attention as any adduced in this topic, that, though the Dygden did sail from the Havannah, and had lost several of her crew during the voyage, yet these men did not die of yellow fever. Even on the supposition that the sickness had been the fever of Havannah, it is difficult to understand how it could operate on the constitutions of the men in such a latitude as that of the banks of Newfoundland. It is also positively stated by the captain of the vessel, that the clothes worn by the two men who died were, with the bodies and hammocks, thrown into the sea.

This condition further implies, that the disorder be capable of being conveyed from the spot in which it is observed first to prevail, either in the persons, the clothing, or the bedding of human beings, or in the furniture of horses, to places in which it has not been hitherto known, and affect the inhabitants.

This test, which it is less difficult to obtain than the last, has never been afforded unequivocally regarding yellow fever. Numerous instances, indeed, have been adduced by Sir William Pym, the French Commissioners, and others of the same side, in which the disease has been alleged to be conveyed in this manner from one place to another. But in every one of these recurs the difficulty, whether the local peculiarities of the town to which the disease was said to be conveyed were not adequate to produce the disease. Thus, without going back to a more remote period than the Catalonian epidemic of 1821, the disease was represented to have been conveyed from Barcelona to the villages of Sans, Sarria, Canet-de-Mar, Sitja, Villa-Seca, &c. and eventually to Tortosa, Asco, and Mequinenza. From the statements of M. Chervin, however, as reported by the Academy, it appears, that every one of the inferences regarding the transmission of the disease to these places were founded on erroneous or uncertain facts, or statements altogether fictitious. Thus it appears, that of the 18 persons who died at Sarria during the Catalonian epidemic, not one had been at Barcelona; and the individual himself, Dr Fabregas, who was supposed to have been at Barcelona, and to have brought it in his own person from a patient seen there, had visited no patient in that city. In like manner, it appears that Antony Salarich, not Celeric, as stated in the Report, who was blamed with having introduced the disease into Sans, and communicating it to his wife, though the latter had, contrary to the statement of the Report, gone frequently to Barcelona during the epidemic, yet never communicated the disease to any one in the village. In the case of all the other places to which the fever was alleged to have been conveyed, the assertions appear to rest on equally unsatisfactory grounds.—Rapport lu à l'Academie Royale de Medecine, &c. &c.

The strongest circumstance, however, by which this whole argument is completely neutralized, is that related by Mr Amiel at the 276 page of this Number, in which it is stated, that, though 5 cases of black vomit occurred in the 12th regiment, the disease did not spread after the regiment was removed to the Neutral Ground, and in which also 92 women and 150 children, who were equally separated from the original source of disease, remained in perfect health.

On this point of the incommunicability of yellow fever from the sick to the healthy, a great number of very strong, if not entirely conclusive, facts have been collected by the industry of M. Chervin.

It is, in the *first* place, remarkable, that, although it has been the constant practice of the inhabitants of towns in the United States to flee to the country as soon as the disease appears, and for those who are attacked to be conveyed to the abodes of their families, yet in no instance has the yellow fever been propagated out of the towns, or in the interior of the country. The most immediate and direct contact has been incapable of communicating the disease in a single instance, according to the incorrect testimony of the physicians of the different towns of the coast from Louisiana in the south, to Maine in the north. Several contagionists even admit that they have never known a single instance of yellow fever being propagated to the country either by conveying the patients or their effects. It is, however, proper to mention, that Dr New, among those who admit this propagation, quotes one fact of which he was witness. Others, for instance Dr Parrish, Barnwell, and Parke, are satisfied with stating in general terms, that there are examples of transmission to the country, or to mention instances in which the yellow fever was propagated, towards the close of last century, to Germantown, near Philadelphia, and at Huntington near New York; but both instances are opposed by other documents.

In the late Gibraltar epidemic, notwithstanding the immense number of persons who took refuge in the shipping in the bay, not a single instance of communication of the disease was observed.

Of much the same nature is the fact, that when yellow fever prevails in a city of the American Union, the inhabitants of the infected district, sick and sound, repair in great numbers to the sound quarters, yet without communicating the disease to those who receive them in their houses, and render them the requisite attentions, though the apartments in which they are accommodated are often small, ill-ventilated, and uncleanly. The disease remains uniformly confined to certain localities, generally low and unhealthy.

Secondly, in the hospitals devoted to yellow fever patients, the attendants of every class have been invariably exempt from the disease, when these establishments are situate beyond the source of the sickliness, and if the attendants did not expose themselves to it. Thus yellow fever is not propagated in the hospitals of Bush-hill, of the City, and of the Marine, near Philadelphia; in those of Bellevue, of Fort Stevens, and of the Marine in the vicinity of New York; nor in those of Norfolk, Baltimore, Providence, Newport, Boston, and New London. In the Antilles, in which it is difficult to erect hospitals beyond the influence of all the local causes of infection, the attendants have either been altogether exempt from the disease, or have not been more generally affected than the rest of the unseasoned population. Yellow fever, for instance, is not propagated in the hospital of Kingston in Jamaica, nor in the military hospital established at Port Antonio, in those of Barbadoes, nor in the hospital of St Ambrose at Havannah. It is further to be remarked, that at Vera Cruz, which is perhaps the most perfect nursery of yellow fever in the whole habitable globe, the idea of the contagious propagation of yellow fever is utterly unknown. Though in this town strangers, both from abroad and descending from the higher grounds in the neighbourhood of Xalapa, are almost sure to be attacked by the disease on entering the city during the season of its prevalence, yet so complete is the conviction of the absence of contagion, that a person walks into any one of the three hospitals devoted to patients of this description, and approaches patients perfectly yellow and vomiting dark-coloured matter from time to time, yet without the smallest apprehension that he is in this manner to receive the disease. When Baron Humboldt visited this city in 1804, he and M. Bonpland were conducted by Don Florencio Perez y Comoto, the enlightened physician of the small hospital of San Sebastian, to the bedside of a dying muleteer, who had been attacked by the disease in crossing the plain between Antigua and Vera Cruz, yet without either of them sustaining any inconvenience from such a visit.

Thirdly, Though, according to the hypothesis of contagion, it might be imagined that persons frequently approaching patients within the range of infection are more liable to contract yellow fever than those at a distance, and not communicating with them, yet this is not the case. Individuals already attacked with yellow fever, or on the eve of being attacked, have been led to prison and undergone the disease without communicating it to their companions in misfortune, though communicating freely with them, and attended by them in close and badly aired apartments. Pregnant females attacked by the disease have been delivered in this state without communicating the complaint to their offspring. Nay, nurses overtaken by the epidemic have continued to suckle their infants during part or the whole of the disease without any inconvenience resulting; and in some instances the living child has been found attached to the nipple of the lifeless mother without being afterwards affected. It must not, however, be denied that this fact, which has been observed also by contagionists, (*vide* Report of Sir James Fellowes, p. 484,) may also be conceived to indicate the unsusceptibility of the infantile age,—a fact remarked by all physicians of whatever opinion.

This objection does not apply to other facts of this nature collected by M. Chervin. Many persons, he informs us, of both sexes, and of all ages, have slept several successive nights with patients labouring under yellow fever, yet without contracting the malady. This circumstance has been principally observed in necessitous families, in which, consequently, the apartments were small, foul, and badly aired. Many physicians, surgeons, and nurse-tenders, have received the matter of black vomit on their clothes, hands, and face, even yet without suffering from it. In Guiana, the Antilles, and the United States, physicians and surgeons have inspected bodies of yellow fever patients, immersed their hands in the fluids, and have breathed the fetid exhalations issuing from the stomach and intestinal tube with impunity. Even exhumation has been practised without bad consequences. In other instances physicians have cut or otherwise injured themselves in inspecting the corpses of persons cut off by yellow fever, and have immersed their hands so injured in the different fluids without bad effects. In this manner, for instance, while inspecting the corpse of M. Montegre, at Port-au-Prince, M. Eymond wounded his finger without suffering any bad consequence. Even Drs Prost and Dorsay of South Carolina, O'Connar of Trinity Island, and Govin of Havannah, with some others, hazarded the bold experiment of inoculating themselves with the blood, the serum, and the black vomit, taken from corpses without any result; and some of them have sacrificed their feelings so far as to taste this ominous secretion. Chervin himself, indeed, has not only tasted but drank the dark-coloured fluids contained in the stomachs of yellow fever corpses, in so many instances, that, in November 1817, they amounted to about 500; yet neither he nor his coadjutors in these revolting experiments sustained the smallest injury.

Lastly, The apparel used by patients has appeared to be equally inoffensive as their persons and corpses. According to the documents collected by Drs Lehman and Mease of the Lazaretto of Philadelphia, immense quantities of bed-clothes, pillow-cases, and similar articles stained with blood, black vomit, and other evacuations, have been touched, handled, and washed by the attendants or private persons without the smallest inconvenience. Beds even on which patients have died have been still, unpurified, occupied by persons in health, or attacked with trifling complaints, without evil result. Thus, M. Bonquin, surgeon at Guadaloupe, was one day obliged, when in perfect health, to sleep in a bed in which an officer had just before died of yellow fever ; and though the clothes and mattress were impregnated with blood and black vomit, he suffered no inconvenience. Similar facts have been recorded by MM. Belloc, Vatable, Allenet, and Chopitre of Guadaloupe. Much to the same purpose are the instances of persons who have worn the clothes, used the same utensils, and drank even from the same vessels with yellow fever patients with impunity. M. Edward Castro relates that a corporal of the regiment of Tarragon, who was in garrison at Havannah, put on, instead of his own shirt, that of a man who had just died of yellow fever without suffering any inconvenience. M. Chopitre witnessed a similar instance at the Military Hospital of Guadaloupe.

These facts, if accurately observed and faithfully reported, as we are in the meantime bound to believe them to be, tend in the strongest manner to disprove the contagious nature of yellow fever in the New World. If they can be verified, which we understand they are in the Spanish peninsula, the question may be regarded as decided.

A second proof of the contagious nature of a disease is the circumstance, that its communication may be prevented by separation or seclusion of the healthy from the sick, and the prohibition of all intercourse. It was at one time imagined that distinct proofs of the efficacy of this method of prevention were obtained ; and the case of Colonel Fyers of the Engineers, and similar ones, were triumphantly quoted as decided proofs of the contagious nature of the yellow fever. Unfortunately, however, we have had in recent epidemics too many proofs of the complete inadequacy of this method, and numerous examples of the appearance of the disease among persons secluded from the sickly in the most rigorous and cautious manner. Thus it was asserted by the Medical Commission already referred to, that in the Barcelona epidemic of 1821, the soldiers attacked by yellow fever were treated apart from the hospital in a ward, in which they were not allowed to communicate with the others. In spite of this precaution, however, according to the almoner, Don Jose Rieira, the disease was propagated to the other parts of the hospital. It is further represented in that report, that the citadel of Barcelona was completely secured by the same means from the disease. It appears, however, from a document of M. Francis Borras, surgeon mayor of the citadel, that yellow fever attacked 13 or 14 criminals confined in the fort. In the late Gibraltar epidemic, though the family of Mr Farquhar adopted measures of seclusion, one of them sickened and died; and in no instance did the seclusive measures appear to diminish the proportion of persons attacked.

Conversely, we find that in situations in which immunity from the disease was ascribed by the Commissioners to isolation and seclusion, this isolation was extremely imperfect, and altogether inadequate to the purpose. Thus the convents of the Capuchins, the Angels, St Theresa, the Hieronymites, and the Carmelites, who are represented to have escaped in this manner, communicated with the city of Barcelona freely as at all other times. In the case of the Capuchins, the friars begged their food as usual throughout the city, infected and sound. The sisters of the Angels distributed daily water, reported specific against the disease, to a great number of persons of all ages and both sexes, some well, others sickly; and those of St Theresa appear to have been equally indiscriminate in their intercourse with infected houses. In the late Gibraltar epidemic, in like manner, previous to the death of Dr Hennen, no seclusion was enforced, and free intercourse existed between the town and the Neutral Ground, (evidence of Mr Fraser and others) yet without increasing the number of attacks thereby, or importing the disease from the former to the latter situation.

These facts show, that at least the doctrine of the contagious character of yellow fever has been received on grounds greatly too slender. They perhaps in the estimation of many may be regarded as adequate to prove that it is in no case, and under no circumstances, contagious; and unquestionably this inference would be quite legitimate if all the facts adduced by the advocates of the contagious propagation were refuted as ably as those of the Commission have been by M. Chervin. Several of those, however, mentioned, especially by Sir James Fellowes, and by Dr Burnet in regard to the Island of Ascension, appear still to be difficult of explanation without the hypothesis of contagion. On this point, however, which it is sufficient to mention, we find it impossible at present to enter.

There is still, nevertheless, one point which may admit of some doubt with impartial and scrupulous judges. Yellow fever in the New World, for instance at Vera Cruz, Havannah, Jamaica, Antigua, Barbadoes, New York, Philadelphia, and similar places, is in no instance known to be contagious or communicable from one person to another. Is it equally incapable of communication in this manner in Europe? On this point we find that the authors of the Report on the Yellow Fever of the peninsula of 1804, Dumeril, Bally, and Nysten, express the opinion that it was contagious in those places only in which its ravages were most conspicuous; and they willingly admit that "it is proved by numerous facts observed, especially at Malaga, Alicant, and Carthagena, that affected persons did *not* communicate the disease in the villages to which they had fled, though the climate there was the same as that of the infected cities."

This opinion, which is given in one of the most temperate and least declamatory reports which have been made on the subject of yellow fever, induced Humboldt, with equal candour and philosophical liberality, while he maintained on the ground of personal observation, that the yellow fever is not naturally contagious, and is never observed to be propagated by contagion in the New World, to admit that it might, under certain circumstances of climate and season, and accumulation of patients, assume a contagious character. His language is so remarkable, and has been so strangely misunderstood, that we must be allowed to give the passage.

"It is incontestable that the black vomit is not contagious at Vera Cruz. In most countries the vulgar regard as contagious diseases which are void of this character; but in Mexico popular opinion does not forbid the unseasoned stranger from approaching the beds of yellow fever patients. No fact is adduced to render it probable that the immediate contact, or the breath of the dying patient, is dangerous to the unseasoned persons by whom he is attended. On the Continent of equinoctial America, the yellow fever is no more contagious than the agues of Europe."

"From the information which I have been able to obtain during a long abode in America, and according to the observations of Messrs Mackittrick, Walker, Rush, Valentin, Miller, and of almost all the physicians who have practised at once in the Antilles and the United States, I am inclined to believe that this disease is contagious in its nature neither in the temperate zone nor in the equinoctial regions of the new continent. I say *in its nature*, for it is not contrary to the analogy presented by other pathological phenomena, that a disease which is not essentially contagious, may, under a certain influence of climate and seasons, by the accumulation of patients, and by individual disposition, assume a contagious character. It appears that these exceptions, infinitely rare in the torrid zone, are presented more particularly in the temperate." After mentioning, in illustration of these views, the opinion stated in the Report of MM. Dumeril, Bally, and Nysten, already adduced, he proceeds to remark, that, in surveying the equinoctial regions of America, the United States, and the southern countries of Europe, where yellow fever has prevailed, we observe, that, notwithstanding the equality of temperature which continues for several months of summer in zones very distant from each other, the disease assumes a different aspect in each. "Between the tropics its uncontagious character is almost universally admitted. In the United States this character is warmly contested by the medical faculty of the University of Philadelphia, and by Wistar, Blane, Cathral, and other distinguished physicians. *Lastly*, As we advance to the north-east, in Spain we find the yellow fever *indubitably* contagious, as is proved by the example of persons preserved by isolation, though in the centre of the malady."

It is probably on this passage that Sir Gilbert Blane has ventured to make the following statement, which would be otherwise quite unintelligible, and altogether irreconcileable with the cautious and guarded manner in which Baron Humboldt has expressed himself above.

"But this," says Sir Gilbert, "is not the only point in which importation and non-contagion have been disjoined; for Baron Humboldt, while he throws great doubts on importation, founding his opinion on the writings of Rush and others to whom he refers, expresses his firm belief in the contagious nature of the Andalusian epidemic, founding it on the very able Report of the Commissioners appointed by the French government to investigate it."—Medical Logic, sect. vi. p. 147.

To make any observations on this passage is perhaps quite superfluous. But while from the former part of the quotation from Humboldt, it is manifest that that author does *not* express his firm belief in the contagious nature of the Andalusian fever, but merely his opinion that yellow fever *may*, under certain circumstances, *assume* the contagious character, and while, in the very report on which Sir Gilbert Blane states this opinion is founded, it is distinctly admitted that the disease was not in all instances propagated by contagion, we confess it requires a mind of a very peculiar construction to adopt the inferences drawn by the author of the *Elements of Medical Logic*.

It may turn out on further inquiry that the opinion expressed so cautiously by Humboldt is well founded, and that the disease may occasionally acquire contagious properties. In the meantime, however, this is to be determined not by collecting the mere opinions of physicians, however able, and counting them like the suffrages of a body of electors, but by observing the circumstances under which the disease is propagated, and especially those under which it seems to spread from sickly localities to remote and healthy districts.

It may naturally be asked, if yellow fever is not contagious, what are the cause or causes of its production and propagation, and what are the circumstances necessary to its developement? On this topic it is impossible at present to enter, nor is it of much consequence to do so. If it be once determined that the disease is not contagious, it follows that all the means of seclusion and separation are utterly useless. The preventive means must then be directed rather to the purification and rectification of the localities in which the disease first appears, and to the removal of the inhabitants to places more salubrious. All *cordons* must be then abandoned, except those which prevent approaching insalubrious districts; and these, if known, will be naturally and instinctively avoided. It is to be feared, indeed, that the sanitary measures by means of *cordons* of troops are directly pernicious, in so far as they confine the population to the infected district. It is at least doubtful whether their effect has not been hitherto ambiguous.

ART. II.—1. *A Dissertation on the influence of Heat and Humidity; with Practical Observations on the Inhalation of Iodine and various Vapours in Consumption, Catarrh, Croup, Asthma, and other Diseases.* By JAMES MURRAY, M. D., Member of the Royal College of Surgeons, London, &c. Longman and Co. London, 1829. 8vo. Pp. 305.

2. *A Treatise on Pulmonary Consumption, its Prevention, and Remedy.* By JOHN MURRAY, Member of various learned Societies. Whittaker and Co. London, 1830. 12mo. Pp. 156.

3. *Two Memoirs read before l'Académie Royale des Sciences, at Paris, on the successful Inhalation of Diluted Chlorine in the early stages of Pulmonary Consumption; as a remedy capable of prolonging life, and of alleviating the distressing symptoms in the more advanced stages of that complaint; with cases illustrating, &c. Translated from the French of M. Gannal.* By WILLIAM HORATIO POTTER, M. R. I. Operative Chemist. Callow and Wilson, London, 1830. 8vo. Pp.

4. *Remarks on a New and Important Remedy in Consumptive Diseases; with a Description of an Apparatus for the easy and domestic employment of a medicated atmosphere in cases of impeded respiration.* By JOHN DODDRIDGE

Humphreys, Surgeon. Kidd, &c. London, 1831. pp. 51. 12mo.

5. *Cases illustrative of the Efficacy of various Medicines administered by Inhalation in Pulmonary Consumption; in certain morbid states of the Trachea and Bronchial Tubes, attended with distressing Cough; and in Asthma.* By Sir Charles Scudamore, M. D. F. R. S., &c. Longman and Co., London. 8vo. Pp. 113.

If patients continue to die of consumption in this kingdom at the rate of many thousands annually, it is not for want of new remedies and new books on the treatment of it. Here are five different methods of cure never thought of before, which have been all discovered, or at least publicly announced, in the course of two years, and which the discoverers, although they do not go so far as to consider them infallible, nevertheless evidently desire the world to believe will make a serious impression on the mortality occasioned by this frightful scourge. *Mr John Murray* proposes to cure it with the chlorate of potass. *M. Gannal* expects to do so by making the patient inhale chlorine. *Dr Murray* entertains equally sanguine hopes of success from the inhalation of iodine. *Sir Charles Scudamore* adopts Dr Murray's proposal, and stamps it his own, by adding to the iodine the aqueous fumes of certain narcotic drugs. And, lastly, *Mr Humphreys* constructs a *Canopy*, under which he shrouds his patient's head and insinuates various medicinal steams, with the conviction that he has thus succeeded in devising an equivalent for the balmy airs of Madeira.

Mr Murray is not of the medical profession, but, stepping forth in the pure capacity of philanthropist, implores merciful treatment at the hands of those on whose province he ventures to encroach. He is consequently not a fair object for any severity of criticism; and therefore all we shall say of his treatise and his treatment is, that we have in vain searched the former for a sufficient body of facts to warrant his sanguine hopes, and have made actual trial of the latter without being able to remark any operation, either therapeutic or physiological, from the doses he recommends. Mr Humphreys may be set aside more summarily; for as he takes care to tell the reader that his canopy is to be found at such a place, and himself at such an hour of certain days of the week, we presume his book is intended simply to advertise who, what, and where he is. The three remaining works, although not equal in point of pretension to the two former, are worthy of some consideration, on account both of their authors and their subjects.

Every person of common sense will allow that there is no disease which may be made the subject of therapeutic experiments with so much propriety as consumption. The hopelessness of every case and inefficacy of every known treatment, wherever the disease has been unequivocally ascertained to exist, will justify every kind of experiment in search of a new remedy. It is likewise reasonable to presume, that if the search is to prove successful at all, there is a greater likelihood of its arriving at so desirable a termination among the class of substances which admit of being directly applied to the diseased organ, than among any other description of articles in the materia medica. For these reasons M. Gannal, Dr Murray, and Sir C. Scudamore, are sufficiently justified in their attempts to discover a remedy among gases and vaporizable substances, which are capable of being brought by inhalation into immediate contact with the diseased structure of the lungs; and we may add, that it was even natural to fix first upon chlorine and iodine,—on the former, because it has been fully proved to possess considerable power in altering the morbid secretions of mucous membranes, and the surfaces of ill-conditioned ulcers,—on the latter, because it has been tried as a remedy for almost all other incurable diseases, and has been found to possess a singular action on morbid enlargements of glands, to which some persons may still, perhaps, believe that pulmonary tubercles bear a distant resemblance.

But the article for the experiment being once fixed on, the same reasons will not warrant the determination, at which all our authors seem to have arrived, to find the said article a useful remedy. In experimenting on a disease, which has hitherto proved almost invariably fatal under every kind of treatment, and of which the diagnosis, at the stage when alone it can be expected to yield to treatment, is uncertain, we should expect that the experimentalist, if he is imbued with the true spirit of philosophical inquiry which appears to be gaining ground rapidly in medical science, would not only proceed to his task with an unprejudiced mind, but likewise give a wide extension to his researches where subjects of experiment are to be had so easily in every quarter, and exhibit his success in a situation where the public might have practical proof of its reality. All this may easily be accomplished with a real remedy, and unless these conditions are observed, the public, like ourselves, will, we trust, place no confidence in the statements of authors, whose manner of procedure entitles them to no better designation than that of hasty projectors.

It was scarcely to be anticipated that one who thought his lucubrations worthy of occupying the time of the Parisian Academy of Sciences,—or another who bears the high title of Honorary Member of Trinity College, Dublin,—not to mention a mere Doctor of Medicine,—should appear so unacquainted with the common rules of evidence as to conceive, that the few imperfect facts contained in their treatises justify the vast and important conclusions which they desire the reader to draw from them.

Dr Murray, talking of the inhalation of iodine, says in his preface, that "if he had not abundant proofs of its value, he would not be the first to propose it;" that "it will sometimes heal if early applied, and will give rest and repose and relief in cases where it is impossible to cure." And subsequently he states in terms still more commendatory, that "it is capable of procuring decided relief from pain, and disposing the patient to repose, when the tubercular nodules have suppurated in the centre, and open into the neighbouring bronchiæ, or when they unite into large abscesses;" that then "the free use of iodine in vapour has improved the condition of the parts, so as to alter the discharge; and where the cavities were not extensive, or the tubercles opening in successive crops, restoration to health has sometimes taken place." P. 255. Yet notwithstanding this affectation of refinement in diagnosis, which no man ever dreamed before of aiming at, and notwithstanding these extravagant encomiums bestowed by himself on his discovery,—what is the amount of the facts contained in his book? So far as we can discover, a single case only, (p. 256,) which we beg to observe no physician could distinguish by the author's narrative from a case of chronic peripneumony, or of chronic pleurisy united with catarrh, and which he winds up by leaving the patient merely in a state of "improvement particularly encouraging." Does Dr Murray seriously expect to force his opinions on the public with such means, even aided by the threats of literary revenge which, with equal good sense and politeness, he announces in his preface as ready for those who do not coincide with him?

M. Gannal, who prefers chlorine to iodine, terminates his essays with conclusions nearly the same with those of *Dr Murray*. He infers from his experience, that the inhalation of chlorine is perfectly safe; that in advanced incurable cases of phthisis it affords relief and prolongs life, and that, "in cases where other medicinal aids were of no avail, it succeeded in effecting a cure in a longer or a shorter interval;" and his translator, echoing and expanding this assertion, observes that *M. Gannal's* memoirs "prove in an incontestable manner, that chlorine gas, diluted with a large proportion of common air, and softened farther by its combination with aqueous vapour, is a powerful therapeutic means of cicatrizing ulcers of the lungs where they exist, and preventing their formation when a predisposition is indicated." But what is the amount of experience to justify this extravagant commendation?

In his first memoir three cases are succinctly related. The first of these, a man of forty, was pronounced by Laennec's nephew to have tubercles in the upper part of the right lung, because he remarked dullness of sound on percussion, cavernous respiration, and crackling râle. After inhaling chlorine for a week, the purulent expectoration became mucous, the oppression of the breathing abated, the diarrhœa ceased, and the appetite returned. In the course of two months more he had slight relapses; but in the end his state improved still farther, the dyspnœa disappeared, the night sweats became less frequent, and the pulse fell to 62. Now, is there evidence in this statement of the man having had tubercular phthisis? And if so, is there proof that he was cured? We suspect, that, in a matter of such vital importance as the main point in question undoubtedly is, the answer must be given in the negative. The stethoscopic signs are scarcely sufficient to prove unequivocally the existence of tubercles; and if we understand the narrative correctly, it leaves the patient coughing, expectorating mucus and sweating occasionally at night.—The second case we are merely told was judged by his physician to be incurable. After the use of chlorine the cough became less frequent, and the sputa, from purulent, became first muco-purulent, and then more nearly mucous. But about this time the patient imprudently exposed himself to cold, became much worse, and soon fell into a hopeless condition,—in which the narrative leaves him.—The third case is also related very imperfectly. The patient had troublesome cough, purulent expectoration, a dull sound on percussion in the upper part of the left side of the chest, and in consequence had been advised to repair to a warmer climate. After the use of chlorine, the cough and expectoration diminished, the appetite and strength improved, the peculiar sound in the left lung ceased, and, at the date of the publication of the memoir, "every thing showed fair for a speedy restoration." Supposing a cure was accomplished in either of these cases, does *M. Gannal* bring forward even presumptive evidence that they were genuine cases of tubercles in the lungs? We must reply, that, for our own part, we should never be satisfied with such a diagnosis. Yet this is the whole amount of the evidence with which the author ventures before the most learned and scientific body in Europe, and endeavours to convince it that he can cure consumption.

In his second memoir, he traces his three cases throughout their farther progress, and adds several more. The two first mentioned in his earlier memoir terminated fatally. The third recovered completely, and went to Belgium, from which his physician writes to *M. Gannal*, that "he enjoys the best possible health in every respect;" and that "it might almost be doubted whether his chest was ever affected, so free at present are his respiration and speaking, notwithstanding rather hard exercise, and a regimen not quite in accordance with the laws of health." The fourth was a case of well-ascertained phthisis, which received some benefit from inhaling the chlorine, but, nevertheless, pursued the ordinary course, and ended fatally. The fifth was considered a case of advanced and hopeless phthisis, where, however, after inhalation was continued for some time, the purulent expectoration ceased, the cough abated, the appetite returned, and the patient regained strength materially, but was not regarded by his physician as freed from his disease. The sixth case is an attack upon a Dr Laroque for not supplying the author, when requested, with the facts required to enable him to draw up an account of an instance of alleged complete cure. In the seventh, relief was obtained from the use of chlorine, and life apparently prolonged, but the termination was eventually fatal. The eighth and last was considered by the medical attendant a genuine case of phthisis. The stethoscopic indications were a mucous râle and wheezing, not far from the right clavicle, with dullness of sound on percussion around this spot, over the whole upper third of that side of the chest. After the use of chlorine, the general phthisical symptoms abated materially, and the patient's strength was greatly improved; but in the right side there was distinct pectoriloquy, so that, although the relater, a friend of *M. Gannal*, seems to anticipate a permanent cure, he does not go so far as to allege that it has actually been effected.

Here then are eight cases; of which four ended fatally; two experienced such amendment as the physician is accustomed to witness from other treatment, but at the date of the publication of the memoir were considered in a doubtful state; one is alleged to have recovered, but the data in support of the assertion cannot be procured; and one (Case 3) is an apparent instance of complete recovery, but the only proof of the existence of tubercular phthisis consists in the presence of the general symptoms of that malady, which it is unnecessary for us to say are considered in the present day wholly insufficient to establish a diagnosis, together with circumscribed dullness of sound on percussion in one side of the chest, which we may also venture to say no stethoscopist worthy of the name will pretend to call sufficient evidence of the presence of tubercles, more especially in a question of so much importance as that of a supposed cure. We see no reason, then, to believe that M. Gannal has even proved presumptively a single case of ultimate and permanent success from his treatment; and it may be well imagined that he has brought forward the most favourable examples which his experience could furnish.

To the memoirs of *M. Gannal* his translator has appended the details of an additional case of apparent cure by *M. Costa*; and it is but justice to admit, that we should depart from that impartiality of judgment which ought always to govern us, if we did not grant that the narrative supplies strong presumption of a complete cure of a genuine case of phthisis having been actually accomplished. On this account we shall present an abstract of it.

The patient was a young lady, twenty-eight years of age, tall in stature, of contracted chest and delicate habit, and of a family in which two brothers had died of pulmonary diseases. She had when *M. Costa* first saw her, frequent cough, copious expectoration of mucus, with lumpy matter, and occasionally blood, great oppression of the breathing on motion, daily rigors, with nightly sweats, loss of appetite, much emaciation, and suppressed catamenia. The antero-superior part of the left side of the chest sounded dull, and presented a complete absence of respiratory murmur. These complaints were of five months standing. From the 12th till the 30th of March no particular change occurred under the ordinary palliative treatment of phthisis.—In the early weeks of April the expectoration became more homogeneous, and exhaled the murine or musty odour which some consider characteristic of the formation of tubercles. At the same time the other symptoms became worse; and at the part of the chest which sounded dull there was a slight mucous râle and imperfect pectoriloquy. At the end of the same month the pectoriloquy was more distinct and extensive, the respiratory sound from the description appears to have been cavernous râle, and over the region of these sounds the sound on percussion was now hollow. Supposing *M. Costa* to be skilful in the use of the stethoscope and percussion, which we have neither means of knowing nor reason to doubt, there is in this narrative as complete evidence of a phthisical cavity having been formed as can possibly be procured.—Seeing the inutility of his previous treatment, M. Costa resorted to the inhalation of the vapour of a warm solution of chloride of lime much diluted. In six days the sputa and sweats were considerably abated, and the former free of fœtor. Five days later the chloride was suspended on account of a sudden increase of fever with pain of chest; and the expectoration soon recovered its original ill-conditioned qualities. On the 20th of May the inhalation was resumed and persevered in till near the end of June. In ten days the expectoration became again less abundant and of better character, and, besides other important ameliorations, the pectoriloquy was more circumscribed. In the middle of June the catamenia reappeared, the night sweats ceased, the expectoration was a clear, insipid, scanty mucus, the pectoriloquy ceased altogether, and the natural respiratory râle began to be heard in the portion of the chest where it was wholly absent two months before. Towards the end of the same month, the whole of this part of the lung was pervious, except "a very small part corresponding to the centre of the second rib, which *M. Costa* looked upon as the cicatrix left by the obliteration" of the cavity.—During all this period the young lady's general health and strength progressively improved; she married in October, afterwards became pregnant, and finally was brought to bed of a fine child, without any injury to her health; so that *M. Costa* thinks himself fully authorized to consider her cure as complete and permanent.

Any objections which we might urge against this interesting case would be called cavilling by the believers in the virtues of chlorine. We shall therefore refrain from starting certain difficulties which might be brought against it as evidence of a cure; and on the contrary will admit, that if *M. Gannal* had not been hurried away by that growing curse of medical literature, the rage for premature publication, but had quietly waited till he accumulated a body of similar facts, he would have gone far to establish the utility of his remedy, and his own claim for immortality, neither of which is likely to receive much support from his Two Memoirs.

Let us now to Iodine and see what *Sir C. Scudamore* has to say of its virtues in consumption. He administers it in the way of inhalation, together with hemlock, stramonium, or hydrocyanic acid, and combines with this treatment the administration of acetate of morphia, digitalis, stimulant friction of the chest, and a careful regulation of the diet and external temperature. This system of treatment, he says, "is remarkably successful in chronic bronchitis, gives relief to the asthmatic patient, proves often curative in cases of phthisis pulmonalis, not become desperate in their nature, and is capable of much useful influence even in those extreme examples of the disease which too probably admit only of alleviation, and seem to bid defiance to the ordinary rules of practice." We shall now state succinctly the facts adduced by him in support of this statement, confining our attention at present to phthisis, in order not to interrupt the thread of our inquiry.

The cases of phthisis he gives are eight in number.—The first, second, seventh, and eighth, were cases of advanced phthisis, in which the inhalation of iodine and hemlock, with the use of morphia internally and stimulant friction of the chest, appeared to afford much relief by lessening the cough, facilitating expectoration, and removing dyspnœa,—but which proved fatal in the usual manner.—The third was the case of a female, debilitated by repeated miscarriages, affected with cough of four years standing, considerable puriform expectoration, and hectic fever, and in whom there was dullness on percussion in the upper part of the right side of the chest, with distinct pectoriloquy at the apex of the lung, and obscure gurgling. The author's treatment having been enforced for three months, she recovered so completely that it was considered unnecessary to continue the inhalation and other remedies, and some time afterwards she was reported to be in perfect health. No account, however, is given of the progress of the stethoscopic indications; and these, as stated by Sir Charles at the commencement, are equivocal.—The fourth was an instance of inveterate cough in a middle-aged female, with hectic fever, extreme emaciation, and difficult expectoration of muco-purulent matter. In the right arm-pit and outer end of the clavicle there was more resonance [of the voice?] than in the corresponding quarters of the left side, more dullness under the right than under the left clavicle, mucous and sibilant râle on the right side, mucous râle on the left side, but no pectoriloquy. After the author's treatment had been put in force for three weeks, "the patient was satisfactorily convalescent;" in the course of a few weeks more she was quite well; and she continued so six months afterwards. Sir Charles thinks the existence of ulceration doubtful in this case, and the existence of tubercles highly probable; but we see no sufficient reason for even the latter opinion. The signs derived from percussion alone are altogether equivocal. Chronic catarrh, and still more this combined with an old circumscribed peripneumony, or a dense circumscribed pleuritic effusion, would afford all the indications given in the narrative. The fifth case was that of a gentleman, who had been liable to winter colds for five years, and who at length became affected with frequent hard dry cough, emaciation, and night sweats. In the upper part of the right side of the chest there was remarkable resonance of the voice, and obscurity of the respiration, with dullness on percussion; while in the corresponding part of the left side these signs were wanting. Under Sir C. Scudamore's treatment the cough almost disappeared, the night sweats ceased, the breathing became free, the respiratory murmur more distinct, and the sound on percussion clear, so that in the course of two months treatment was discontinued. Sir Charles designates this a case of "chronic cough apparently depending on tubercular irritation;" but the signs are surely much too insignificant to justify even this diagnostic opinion.—Case sixth is entitled "tubercles in each lung; great probability of an ulcer at the apex of right lung." The subject was an elderly gentleman, whose pulmonary complaint commenced with considerable hæmoptysis, and who eventually had an imperfectly-formed hectic, with night perspirations, frequent cough, and copious greenish expectoration. There was general dullness on percussion over the right side, especially at the upper part, and also at the upper part of the left lung; and there was distinct pectoriloquy at the apex of the right, with doubtful pectoriloquy at the apex of the left lung. Under the usual treatment the sputa became clear mucus, and the night perspiration ceased in three weeks, while at the same time his general health and strength improved amazingly. His convalescence went on from that time steadily, and at the end of four months he could walk easily six or seven miles. This case the author "adverts to with infinite satisfaction as proving the great benefit of iodine inhalation," and as one in which he "had the fullest persuasion of the existence of tubercles, and could scarcely doubt the presence of some ulceration." But we apprehend no cautious and practised stethoscopist would come to such conclusions from similar data.

These are the facts upon which Sir C. Scudamore founds his conviction of the sanability of consumption by the inhalation of iodine and certain narcotic vapours. Of the eight cases, four proved fatal; and in the remaining four the existence of tubercles and tubercular cavities is considered even by the author only probable, or at best very probable. Granting that he is correct in considering their existence probable, we could scarcely have believed that a member of the regular profession, of the years and station of the author, could have stepped before the public with this as the sole proof of his having discovered a remedy for a disease hitherto considered almost incurable. Viewed according to the principles of evidence, four probable cures constitute no proof whatever. Besides, the grounds on which he considers the cures probable may be seriously called in question. The conclusiveness of the cases depends mainly on the skill with which the stethoscopic signs and those drawn from percussion of the chest were elicited. Now we conceive that a careful investigation of his narrative will very nearly convince any person practised in the employment of these diagnostic means, that, in the present instance, the requisite skill was not possessed by the observer. Among other proofs of this, it will be remarked, that in relating the indications of cavities in the lungs, he scarcely ever mentions any other sign than pectoriloquy and dullness on percussion. Now, he ought to be aware, that without other signs, these two are extremely fallacious,—that in many individuals it is very difficult to distinguish between bronchophony and pectoriloquy,—and that there is scarcely a single case in his book in which his supposed signs of a cavity in the lungs might not have arisen from simple condensation of a part of the organ in the neighbourhood of a bronchial tube of moderate size. For one who appears to have paid some attention to the subject, it is a strange omission that no mention is made in a single instance of the characteristic signs,—cavernous respiration, and cavernous râle.

Let it not be supposed from the terms in which we have commented on the treatises of Dr Murray, M. Gannal, and Sir C. Scudamore, that we hold their labours and the remedies they recommend as of no value. On the contrary, we advise every reader to peruse what they have written; for undoubtedly they make out a strong case in favour of the palliative effects of chlorine and iodine in consumption, and of the propriety of subjecting both to farther trial, as the means of accomplishing a cure. They farther show, more especially Sir C. Scudamore, that the inhalation of the vapours of these substances, united with other vapours of the narcotic class, may be resorted to with success in other diseases of the lungs, such as chronic bronchitis and all the variety of maladies usually comprehended in vulgar speech under the name of asthma. In such diseases, they show that iodine in particular facilitates expectoration, brings it to the state of clear mucus, lessens cough, diminishes dyspnœa, and promotes sleep and appetite. These objects may certainly be in general attained by other means. But it is well for the practitioner to be put in possession of new resources, especially if the new remedy be so much superior as its proposer conceives, in celerity and efficacy of operation.

But we would request that those who, in consequence of what has been already done, may feel encouraged to make farther trial of the said remedies for the cure of consumption, will consider they are experimenting on a disease which rarely ends otherwise than fatally,—that therefore they must not suppose the public or their professional brethren will be imposed on by the pompous announcement of extensive conclusions founded on a handful of imperfect facts,—that they will make their observations on a scale of extent conformable to the facility of finding proper objects, as well as the vast importance of the result which every one must anxiously hope for,—and that their observations be all conducted with an undeviating attention to the whole means of diagnosis which the late researches of pathologists have placed in the hands of the physician.

The method of administering chlorine by inhalation consists, according to M. Gannal, in adapting to a pint bottle with a wide mouth, a cork containing two glass tubes open at both ends, and nearly half an inch in diameter; of which one just enters within the cork, and has a rectangular bend outside to enable the patient to hold it conveniently with his lips; while the other is straight, and plunges under the medicated fluid in the bottle. The liquid should be about four ounces, and ought to contain from five to ten drops of a concentrated solution of chlorine, which dose may be progressively increased even so high as twenty-five drops, and may be used six or eight times a-day. The water must be heated to 100° or 110°, and preserved at that temperature during inhalation by covering it with some woollen article.

As to the mode of administering iodine, Sir C. Scudamore simply informs his brethren that he dissolves the iodine in a solution of hydriodate of potass, and adds a little alcohol when he dilutes this solution, in order to prevent the iodine from being thrown down. But he does not consider it safe to detail his precise method or the proper dose, because he professes to be afraid that patients might be inclined to make experiments on themselves, without the direction of a physician, and so incur much risk by reason of the great activity and poisonous qualities of the drug. This is in our humble judgment a very unnecessary exercise of caution. Are not patients put in possession of the means of making such experiments when they please with fifty other drugs of even greater potency than iodine? We fear there are many among Sir Charles' readers who will be led by his silence to a very different interpretation from what he apparently intends should be put on it. Dr Murray uses a solution of five grains of iodine in two quarts of water at the temperature of 160°; and the patient is instructed to breath over a vessel containing the solution, the head and vessel being covered over. A much better plan will be to use such an inhaler as that recommended for chlorine by M. Gannal; and from what we have ourselves observed in the healthy subject, we should imagine that a solution of one grain of hydriodate of potass and a quarter of a grain of iodine in four ounces of water would be sufficient to begin with.

ART. III.—1. *Physicalisch-Medizinische Darstellung der bekannten Heilquellen, &c.—A Physico-Medical Description of the known Mineral Springs in the chief countries of Europe.* By E. OSANN, Professor of Medicine, Berlin. *Part First.* Dümmler. Berlin 1829. Pp. 461. 8vo.

2. *A Treatise on the Mineral Waters of Harrowgate and its vicinity.* By ADAM HUNTER, M. D. Physician to the Ge-

neral Infirmary, House of Recovery and Public Dispensary, Leeds, &c. Longman and Co. London, 1830. Pp. 138. 12mo.

OF the two works here announced, the former is the first volume of a systematic description of all the known mineral waters of Germany, Switzerland, France, Italy, and Great Britain, by Professor Osann, one of the most distinguished professors of the university of Berlin. The volume now published is divided into three parts. In the first the author treats generally of the physical and chemical properties of mineral springs in their undecomposed state,—of the proximate and ultimate ingredients of which they are compounded,—of the physical properties and composition of other terrestrial and atmospheric waters in relation to those of mineral springs,—and of the various establishments for the sale of artificial waters, the mode of preparing them, their properties relatively to the natural waters they are intended to imitate, and the respective advantages in employing the natural and artificial kinds.—In the second part of the volume Professor Osann treats of the source of mineral waters, of their situation, and of the climate of their vicinity.—In the third part he first enters fully into the different kinds of mineral waters, distributing them into seven kinds,—the chalybeate, sulphureous, alkaline, magnesian, Glauber's-salt, common-salt, and acidulous waters; and under each of these heads he describes the chemical properties of the genus, its different species, its physiological effects, and its application to the treatment of diseases. He next proceeds to give a brief summary of all the mineral springs of note in Germany, France, Switzerland, Italy, and Britain. He then describes the several forms in which mineral waters are used, and in this section treats of their internal use and the mode of transmitting them to a distance from the spring,—of their employment in the form of common bathing and that of chamber-baths,—of their application in the form of *douche*, with some remarks on the effects and different kinds of the douche,—of their use in the form of gaseous or vapour-baths and vapour-douche,—and lastly of their employment in the form of mud-bath. After this a distinct chapter follows on the principal mineral mud-baths in Europe, distributed into sulphureous, ferruginous, carbonaceous, saline, common earth, and gelatinous mud-baths. The next chapter treats of the collateral particulars to be observed in applying mineral waters to the cure of diseases. Here the author first describes the active or complete treatment, the mild and incomplete treatment, and the mixed treatment, or that in which the use of mineral waters is united with other means of cure,—then proceeds to consider the preparatory and subsequent treatment,—next the rules to be observed in employing mineral waters,—and lastly the rules required for the administration of the particular kinds. The last chapter of the volume contains a general review of the literature of the subject, commencing with the history of mineral waters among the Greeks and Romans, and tracing it downwards through the middle ages, and successively through the fifteenth, sixteenth, seventeenth, and eighteenth centuries to the present time.

In the part of the work which is to follow, *Professor Osann* proposes to give a detailed account of each mineral water mentioned in the short summary which he has introduced into the present volume. As the whole may be expected to appear very soon, we shall delay taking full notice of it till its completion. and be satisfied in the meantime by the present simple announcement. The author has undertaken a task of great labour and no little delicacy. For the materials which must come under his notice are almost unbounded; and the value of a considerable part of them extremely doubtful. We allude especially to the documents that have been published in praise of the virtues of the several noted springs in Europe; which, if they even but approached the truth, would lead us to wonder why mineral waters were not made long ago the only articles of the Materia Medica. If his undertaking, however, be accomplished in the manner to be expected from the high professional reputation of the author,—if it be completed in the same spirit of patient research and cautious judgment which is manifested in what is already before the public,—we have no hesitation in declaring that his work will be one of the most important additions made to the literature of therapeutics since the present century began, and that, for the sake of a highly interesting, but in Britain much neglected, branch of medical practice, we hope to see it speedily appear in an English dress.

It is singular that, although mineral waters are met with in few countries more abundantly than in Britain, and are nowhere more frequented by all ranks and descriptions of patients, there is not only no good systematic account of them yet in print, but even scarcely more than an incomplete monograph on a few of the oldest and most popular; and in consequence, we presume, chiefly of this deficiency, there prevails among practitioners at large a lamentable degree of ignorance as to their properties and precise therapeutic applications. The spirit of inquiry, however, which for some years past has been most successfully directed towards this subject abroad and especially in Germany, is now beginning to manifest itself among our own countrymen. Several chemists of distinction, among whom Dr Thomson stands conspicuous, have devoted a portion of their time to the analysis of the British waters; and we have no doubt that, ere many years pass, good practical dissertations will appear on the medicinal virtues of all the frequented springs in the kingdom.

The Treatise of *Dr Hunter*, of which we purpose now giving some account, is undoubtedly one of the best monographs that has been published in modern times in this country. It is, as every treatise of the kind ought to be, partly professional, partly popular,—intended for the amusement and instruction of the visitors of Harrowgate, as well as for the information of his brethren at a distance; and in both points of view appears to us well calculated to attain the author's object.

After some general observations on the site of Harrowgate, its history, the scenery of the neighbourhood, and the peculiarities of its climate, he gives a short account of its geognosy from the researches of a mineralogical friend, which moderates considerably the enthusiastic encomiums of former writers on that subject; then a brief view of its botanical riches, which appear abundant; then a statement of its population, which amounts to 4000; then a list of its public institutions, among which an hospital for the poor who use the waters, containing thirty beds, is the most remarkable; then an estimate of the visitors, who are believed to amount to the enormous number of 10,000 annually; and lastly, an apparently well deserved tirade against the natives who, in return for the public patronage they have received, have yet done nothing for the embellishment or commodiousness of their town.

The springs in and around Harrowgate are extremely numerous. But according to Dr Hunter the Tewit Well at the eastern end of Harrowgate was first discovered, although it is now neglected and ruinous in consequence of more powerful chalybeates having since come into notice. It was discovered in 1571 by a Sir Thomas Slingsby, who was struck with its resemblance to the celebrated Sauvenière fountain at Spa, and soon convinced himself and the vicinity that its virtues were not inferior. Other springs were discovered not long afterwards, and down to a very recent date new springs have every now and then been found out and brought into repute. In fact, the whole country in the neighbourhood of this remarkable spot furnishes mineral springs of one kind or another, when they are diligently sought for.

The springs which are at present known and more or less frequented are six sulphureous, five chalybeate, two saline, and one saline-chalybeate. The sulphureous springs are, 1. the old

Sulphur Well; 2. the Crown Spa; 3. the Crescent new Pump; 4. the Crescent House Pump; 5. the Starbeck Spa; and 6. the Hospital Well. The composition of these per gallon, according to Dr Hunter's analysis is the following, the numbers under the head of gaseous contents being expressed in inches, and under that of solid contents in grains.

	Gaseous Contents.				Saline Contents.					
					Chloride of			Bicarb. Soda.	Sulph. Soda.	Total.
	Sul. H.	Car. Ac.	Car. H.	Azote.	Sod.	Calc.	Mag.			
1.	15.6	2.7	6.8	8.8	867	8.7	45	20		1016
2.	21.6	4.3	5.8	4.3	802	77.5	38.6	28	0	960
3.	5.7	3.4	4.8	10.1	462	47.2	21.8	11	14	556
4.	3.	3.	2.	12.	280	23.2	17.2	4	0	324
5.	5.	8.3	0.	11.7	122	10.	8.2	3	2.5	146
6.	4.5	5.4	0.	8.1	329	27.6	16.8	3	6.	384

The chalybeate springs are 1. Oddy's Pure Chalybeate; 2. Old Spa; 3. Tewit Well; 4. St George's Well; and, 5. Starbeck. Their composition is as follows:

	Oxide Iron.		Chloride of		Sulph. Soda.	Total.
		Sodium.	Calcium.	Magnes.		
1.	1.8	5.0	6.0	4.0	3.5	21
2.	2.5	Remainder carbonate of lime.				10
3.	2.0	——— earthy salts.				10
4.	1.	——— the same.				10
5.	0.75	—— chiefly chloride of sodium.				18

The saline springs are the Crescent Old Well, the composition of which, as determined by Dr Hunter, is 107 chloride of sodium, 27 sulphate of soda, 10.1 chloride of calcium, 7.4 chloride of magnesium and 4 carbonate of lime,—and the Dropping Well, which contains 132 sulphate of lime, 23 carbonate of lime, 11 sulphate of magnesia, and 6 carbonate of soda. Lastly, there is a saline chalybeate of great strength called Oddy's Saline Chalybeate, which contains 5.3 oxide of iron, 576.5 chloride of sodium, 43 chloride of calcium, and 9.65 chloride of magnesium, in all 636 grains of solid ingredients per gallon. The last is considered a spring of great value, because its strong saline impregnation tends to prevent the tendency which pure chalybeates have to induce constipation.

After a very interesting account of the discovery of these springs, of their relation to the adjoining soil and rocks, and of the improvements that have been made for the accommodation of the public, the author proceeds to give general directions for the use of the Harrowgate waters, then some account of their constitutional effects, next a description of their employment in the form of baths; and he closes the treatise with some observations on the diet and exercise which should be observed by patients.

He recommends that the water should be always drunk if possible at the spring, before breakfast, in two or more draughts of half a pint each with intervening exercise,—that laxatives be taken along with the chalybeate waters, but none with the saline or sulphureous except at first when the bowels, from long continued disorder, are unusually torpid,—that those who experience uneasiness from the large bulk of cold water take part of it warmed, the mineral water if a chalybeate, being of course preserved cold and mixed with warm water immediately before drinking,—that a course of the sulphureous waters be preceded in the plethoric by low diet, laxatives, or even sometimes bloodletting, to prevent the determination to the head which they are apt to induce at first in those unaccustomed to drink them, and that the nausea, flatulence, and oppression at the stomach, which others experience, be corrected by carminative aperients or a few drops of ether, ammonia, or some other antispasmodic,—that the employment of the chalybeate waters be preceded by a proper regulation of the stomach and bowels, perhaps through means of the sulphureous water,—that when the chalybeate course is begun, the dose taken be about a pint, three times a-day, morning, noon and evening,—and that the effects of this water be diligently watched, for if it does not excite a sense of warmth in the stomach, and improve in a few days the appetite, digestion and spirits, it will do no good, and in all probability much harm.

In the section which follows on the constitutional and therapeutic effects of the Harrowgate waters, we confess we have been somewhat disappointed. The observations on their constitutional effects are meagre; and the catalogue of diseases in which they are useful, although the very reverse of meagre, is nevertheless perfectly bare. In fact the whole amount of the information communicated is, that, by a judicious combination of the waters with one another or with other treatment, there is scarcely any chronic disease which is not frequently subdued. We imagine that an acute physician, as the author is well known to be, living near the spot, and enjoying ample opportunities of witnessing the progress of cases, could supply his professional brethren with much excellent instruction as to the physiological operation of the different waters on the healthy subject, and the circumstances connected with their therapeutic action. Without some such information we cannot hope ever to arrive at a scientific knowledge of mineral waters as a remedy in diseases, but must continue to apply them as at present by the worst of all empirical principles,—namely, the chance of their doing good after every other treatment has failed.

The succeeding chapter on Baths is chiefly a popular disquisition on baths generally. Here, as under the head of the effects of their internal administration, we look in vain for some light on the operation of the Harrowgate waters in the cure of diseases. The same remarks apply also to the concluding chapters on Exercise and Diet. The reader might naturally expect information on the practice prevalent at Harrowgate in regard to the diet and exercise of invalids while using the waters, and some notice of the author's opinions and experience as to the proper rules to be followed. But Dr Hunter is silent on both these points.

In conclusion, then, while we repeat the opinion already expressed, that this treatise is one of the best we possess on any of the mineral springs of Britain, and may also add, that it corrects many erroneous statements regarding those of Harrowgate to be found in various descriptions of them since their earliest discovery down to the recent essay of Scudamore,—we nevertheless cannot quit the subject without observing, that the work does not altogether come up to our ideas of a complete and practical monograph on a mineral water. The analysis of the different springs of Harrowgate is full and precise, and appears to have been executed with much care. The historical part of the work is also clear and well told; and the description of the general features of the surrounding country is lively and interesting. But the strictly practical part is defective. We are far from supposing that this defect is to be supplied by a narrative of cases of the various diseases cured by the use of the Harrowgate waters. It is too well known to all professional men how fallacious are the results which such narratives present, and a treatise like the present would become by a due sprinkling of cases of the kind little else than a quack advertisement. But we conceive it would have added considerably to the professional character and practical utility of his work, if Dr Hunter had given a circumstantial account of the constitutional effects of the different kinds of water at Harrowgate, an enumeration of the more important diseases in which, according to his own experience, there is substantial evidence that the Harrowgate waters are useful, a statement of the circumstances which accompany their administration in successful cases, and a better view than he has yet given of the *juvantia* and *lædentia* in the articles of Diet and Regimen. A statistical account of cases, such as the German physicians at watering places now frequently publish, if it could possibly be attained, would be a very valuable and interesting document. But it is to be feared that this is out of the question. We hope the author will keep these strictures in view when he proceeds to his next edition, which it is to be supposed that the 10,000 annual visitors to Harrowgate will speedily enable him to produce.

ART. IV.—*Inquiries concerning the Intellectual Powers, and the investigation of Truth.* By JOHN ABERCROMBIE, M. D. &c. Edinburgh, 1830. 8vo. Pp. 435.

THOUGH it is easy to see that in modern times physicians are desirous to renounce all connection with psychology, it is not less certain that this conduct is sanctioned neither by precedent nor by the reason of the thing. There is indeed a class of practitioners, neither few in number nor inconsiderable in estimation, who are proud to inform the world that they are practical persons, and to impress on every one around them the fact, that they neither know nor care for any species of knowledge beyond the strict pale of medicine and its practical applications. Every thing which has the air of philosophy they represent as not only useless but pernicious to the genuine physician, by diverting his attention from the business of his profession, and by unfitting him for those prompt and practical applications of his knowledge which are indispensable to the successful exercise of the art. By such persons an invidious line is drawn between theory and practice, the object of which is to throw a degree of discredit and contempt, not only on all the theoretical branches of medical knowledge, but on all those collateral sciences, in which it is pretended it is impossible to recognize any practical application of the healing art.

In this general proscription, of course, the philosophy of the human mind receives its full share of denunciation; and if to such branches of science as physiology, general physics, geology or mineralogy, the medical practitioner can say *cui bono* with any share of justice, much more forcibly perhaps may he apply the same mode of reasoning to that science which proposes to introduce him to the oft repeated discussions about the constitution of that immaterial essence which cannot be made palpable to sensible demonstration, the existence and nature of the mental and moral qualities, the number of original faculties and powers, and the question, whether they are merely modifications of one general faculty, or altogether distinct in operation and property?

Objections of this description, however, which it would be easy to show are neither imaginary nor overrated, originate in superficial and partial views, and are the usual stale arguments by which narrow minds undertake to justify indolence and ignorance, and not unfrequently incapacity. It is idle to recal the oft repeated argument of the mutual connection of the sciences, and the assistance which those, apparently the most remote from each other, have in the hands of ingenious men been enabled to give and receive. This may be admitted, because, if not too obvious to be denied, it is illustrated by so many proofs, that, to question its justice, argues either gross ignorance or wilful obstinacy. But the conviction of its truth is followed by no practical result; and when it does produce something like result, the effect shows that the true mode of applying the principles has been misunderstood.

It is further to be remarked, that many of the objections by which medical practitioners attempt to underrate the study of psychology are derived not from the use; but the abuse of that science, from improper or misplaced applications of its doctrines, and from erroneous views of the powers which it affords. Such objections carry with them their own refutation; for they imply that the conditions are violated according to which only the science can be useful. If a psychological system is false, or founded on erroneous principles, it is equivalent to none at all, and consequently can be of no use to any science whatever. If its principles are applied to subjects to which they are unfit, it is evident that they have been misunderstood. If a psychological system like those of the scholastic ages can do no more than repeat truisms, and draw trivial and foolish distinctions, or lead the inquirer into endless speculative discussions of no practical utility, no one can doubt that the sooner it is abandoned the better, and that to cultivate such an unsubstantial and barren field is neither profitable nor amusing.

Objections of this description are no longer applicable to the psychological systems of the present day. After a variety of vicissitudes, chiefly occasioned by carrying metaphysical speculations beyond their legitimate limits, and without due regard to the limited faculties of man, by the cautious application of the same general principles which are most successful in the physical sciences, the philosophy of the human mind has at length acquired a character of utility which renders it no longer an instrument of idle disputation or fanciful sophistry. It is indeed one of the peculiar characters of modern metaphysics, that it is impossible to employ the human faculties in the successful cultivation even of the physical sciences, without collecting numerous facts which tend to throw light on the constitution of the human mind; and that some of the most remarkable illustrations of the best modes of reasoning and investigating truth are furnished by facts derived from the phenomena of the material world. What a beautiful example, for instance, of the analytical mode of investigation is furnished by the experiments and observations which demonstrate the operation of atmospheric pressure, from the phenomena of the common sucking-pump to the exhausting effects of the air-pump. Chemistry furnishes perhaps a still greater number of instances of the same description. The doctrine of oxidation, at one time so obscure, but now so simple by the number of instances in which the same agent produces different effects, illustrates forcibly the effect of analytic induction and experimental proof; and the still more extensive generalization of the oxidation of the metalloids in the production of alkalies and earths, is one of the strongest proofs of the utility of this mode of inquiry. The phenomena of the communication of heat, and the laws which regulate its transition and effects in different bodies, are further examples, which show how necessary is sound reasoning in the prosecution of physical inquiries.

There are particularly two circumstances which require the accomplished physician to bestow some attention on this subject. In the *first* place, at all periods of the history of medicine we find that the prevailing psychological systems have exercised an influence more or less considerable, and more or less direct, on the doctrines taught by physicians. *Secondly*, the mode of investigation in the physical sciences generally, and in their application, especially to physiological science and medicine, and the mode of estimating the weight of evidence in various complicated and ambiguous medical questions, renders it requisite to have some knowledge of the constitution of those powers which are employed in the investigation of truth, and of the best methods of employing them unfettered by prejudice or other sources of fallacy, in discovering the true relation of different circumstances amidst those complicated combinations in which they are presented by nature.

Various authors, accordingly, have at different periods attempted to introduce a better system of logic into medical reasoning, and to rectify the abuses resulting from imperfect acquaintance with the mode of inquiry and the value of evidence. One of the first, and certainly not the least able, was Dr John Gregory, who, in his lectures on the duties and qualifications of a physician, has laboured to impress the necessity of accurate observation, faithful records, and sound inferences. The Elements of Medical Logic by Sir Gilbert Blane is a valuable compendium of very judicious observations on the application of reasoning to the difficult and ambiguous parts of medical science. It would, nevertheless, have been greatly more useful than it is, had the author not introduced the discussion concerning the contagious nature of the yellow fever, in which it is easy to predict, from the beginning of that discussion, how it was to terminate. This might be an ingenious method of demonstrating the contagious character of the disease; but when it is remembered that it is introduced solely with the view of showing the bad reasoning of all those who denied that yellow fever was a contagious disease, it was at least out of place in a treatise on the mode of reasoning in medicine. Lastly, Vincenzio Lanza, a professor in one of the Italian universities, published some years ago a work on medical logic, in which he considers more fully and minutely than any author has yet done, the use of experience and induction in investigating the theory of diseases and of remedies, and especially that most useful induction which he names *clinical*, and which can be put in practice only at the bedside of the patient.

We propose to avail ourselves of the present opportunity to make some observations on the advantages which an acquaintance with the best systems of psychology may furnish to the cultivation of medicine as a science and as an art. In doing so, while the work of Dr Abercrombie must serve as the general subject of observation, it is proper to remark, that, though it contains nothing absolutely new, it exhibits a very just exposition of the most approved doctrines regarding the intellectual constitution of man, as explained by Locke, Reid and Stewart. The want of novelty in doctrine and principle, however, is a positive advantage in this work; for while it appears to have been the feeling of the author that he could not do better than reproduce and explain the doctrines of these able psychologists, his immediate object is to show his readers in what matter our knowledge of the intellectual constitution may be applied to the cultivation of the science of medicine, and the improvement of our knowledge, by accurate observation and sound reasoning. In undertaking this task, Dr Abercrombie appears to have been perfectly aware of most of the prejudices above-mentioned, and to have had particularly in view the mistakes and defects which may be traced either to the imperfection of our psychological systems or their misapplication.

The application of metaphysical science to medicine cannot be said to be direct; but its indirect uses are manifold. There is no branch of human knowledge in which the mind of the student requires to be so thoroughly divested of prejudice,—to be so free from the operation of fallacies of different kinds,—to be so capable of estimating the weight of evidence of different kinds,—to be so acute in detecting sophisms,—and, amidst a multiplicity of conflicting and complicated circumstances, distinguishing the true and efficient causes from the false and inadequate. Though it would be absurd to expect that metaphysical studies can insure these effects, the intellectual cultivation which their study implies would certainly render the inquirer much more dexterous and sagacious in his researches, and much more accurate in his conclusions, than he would be without such discipline. How often have we occasion to remark, that matters of opinion are stated as matters of fact; and that an author, instead of limiting himself to a strict and faithful description of what he observes, introduces, apparently unconsciously, his own opinions and inferences? In such a mind it is impossible to doubt that no distinct line had ever been drawn between what is *observed* and *actually exists*, and what the observer himself *imagines* to exist. In perusing, for example, the writings of Sydenham, who is almost universally adduced as the father of observation in modern times, how many instances occur in which he associates in his descriptive sketches his own opinions, or those of the time with the facts which he records. The history of medicine, and the annals of the rise and decline of successive theories, show how deficient were their authors in all the requisites of accurate observation, sound reasoning, and legitimate deduction. It is the uniform practice of every one who comes forward with a new system, to begin by showing the insufficiency and errors of those of his predecessors, and after he has said enough to persuade his readers that a new one was indispensable, he proceeds to explain his own, which he imagines will fulfil every requisite. Yet at no very remote period the fallacy of this also is exposed by some new teacher, who in like manner substitutes a theory equally perfect, and which nevertheless is destined to undergo the same fate. It is impossible, would the impartial inquirer argue, that any of these systems can be perfect, however true they appeared at the period when they were first proposed. Fallacy and error,—erroneously observed facts,—false reasoning and illegitimate deduction must exist somewhere. What are the means by which these deficiencies may be detected? Are there any means by which they may be guarded against? Such very nearly may be said to be the reflections which occur to every philosophic mind on surveying the melancholy ruins of the systems of Boerhaave, Hoffmann, Stahl, Bordeu, Cullen, Brown, and Darwin.

In the application of the principles of psychology to medicine, either in the way of discovering truth, or for the purpose of illustrating the natural history of the mind itself, and of the morbid conditions to which it is liable, the principal difficulty consists in the want of that steadiness, uniformity, and certainty which is recognized in the phenomena presented by the physical sciences. In mechanics the several phenomena are regulated by laws which can be estimated accurately either in number and quantity or by experiment. The law of attraction, varied in numerous modes according to the figure and size of the bodies, produces all those complicated yet regular motions which are observed in the planetary system; and even those irregularities which made Newton and some of his followers believe that the system carried with it the principles of its own destruction, have been demonstrated to be periodical only, and to indicate rather the self-adjusting power of its constituent parts. In chemistry, in like manner, all the phenomena may be predicted according to certain given principles and conditions; and so long as these are not violated, the experimenter and observer may reason with perfect certainty as to the results.

One of the most striking examples of the uniformity and certainty of phenomena in the exact sciences is the certainty with which the skilful navigator can, by means of lunar observations, rectify and calculate his course in the ocean between two points separated several thousand miles. The following instance has the advantage of being recent and forcible. "Captain Basil Hall sailed from San Blas, on the west coast of Mexico, and after a voyage of 8000 miles, occupying 89 days, arrived off Rio de Janeiro, having in this interval passed through the Pacific Ocean, rounded Cape Horn, and crossed the south Atlantic, without making any land, or even seeing a single sail, with the exception of an American Whaler off Cape Horn. Arrived within a week's sail of Rio, he set seriously about determining, by lunar observations, the precise line of the ship's course, and its situation in it at a determinate moment; and having ascertained this within from five to ten miles, ran the rest of the way by those more ready and compendious methods known to navigators, which can be safely employed for short trips between one known point and another, but which cannot be trusted in long voyages, where the moon is the only sure guide. The rest of the tale we are enabled by his kindness to state in his own words.—'We steered towards Rio de Janeiro for some days, after taking the lunars above described, and having arrived within fifteen or twenty miles of the coast, I hove to at four in the morning till the day should break, and then bore up; for although it was very hazy, we could see before us a couple of miles or so. About eight o'clock, it became so foggy that I did not like to stand in farther, and was just bringing the ship to the wind again before sending the people to breakfast, when it suddenly cleared off, and I had the satisfaction of seeing the great Sugar Loaf Rock, which stands on one side of the harbour's mouth, so nearly right ahead that we had not to alter our course above a point, in order to hit the entrance of Rio. This was the first land we had seen for three months, after crossing so many seas, and being set backwards and forwards by innumerable currents and foul winds.'"*

In those branches of science, on the contrary, in which we have to reason regarding the motions and actions of living bodies, we no longer observe this uniformity and steadiness, and we find that we cannot calculate with the same certainty on the course of phenomena themselves, or on the effects of those agents by which their course is occasionally interrupted. The ordinary mode of explaining this irregularity is by referring it to the influence, either of the vital principle or of the nervous system, or of both. The cause of this irregularity perhaps is of little moment. The fact is certain, and has been admitted by the most intelligent observers, both in the profession and out of it. It was the inability to recognize the prevalence of this irregularity which so far misled the mechanical physicians, and made them believe, that, in the treatment of diseases, they could calculate with mathematical precision on the production of certain effects. It was the merit of Stahl and Hoffmann, however, to recognize the peculiarity of the phenomena exhibited by living bodies; and if Haller, though a pupil of Boerhaave, was obliged to refer to irritability most of those phenomena which could now be explained on mechanical principles, the controversy between him and Whytt was attended with the happy effect of leading the minds of physicians to the study of vital phenomena as totally distinct from physical.

The first author, however, who appears to have attempted to place this peculiar character of the medical sciences in its true light is Cabanis, who ranks medicine with ethics in this respect. "Unfortunately," he remarks, "these favourable circumstances do not attend the study of several subjects that form an important part of our knowledge. The objects, for example, to which medicine and moral philosophy are directed, present many serious difficulties. The sciences of medicine and ethics, accordingly, must remain much longer in a state of infancy; or at least their principles must of necessity be longer in acquiring that evidence and solidity without which they can scarcely be regarded by exact minds as forming real bodies of science." This difficulty is recognized and still more distinctly adverted to by the late Mr Stewart, who places medicine in this respect on the same footing with the science of political economy, and who ascribes the imperfections of medicine chiefly to the mistaken ideas of what experience is in medicine, and to the tendency which exists to give a hypothetical turn to the facts observed.

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* Discourse on the study of Natural Philosophy. By John F. W. Herschel, A. M. P. 28. London, 1830.

This view is adopted by the present author, who expresses himself on this point in the following terms, which may be regarded as the general statement of the great object of inquiry.

> "As examples of the uncertain sciences, therefore, we may mention medicine and political economy; and their uncertainty is referable to the same sources, namely, the difficulty of ascertaining the true relation of things, or of tracing effects to their true causes and causes to their true effects; and the intervention of new causes which elude our observation, while they interfere with the natural tendencies of things, and defeat our attempts to produce certain results by bringing them into action. The scientific physician well knows the difficulty of ascertaining the true relations of those things which are the proper objects of his attention, and the uncertainty which attends all his efforts to produce particular results. A person, for example, affected with a disease, recovers under the cure of a particular remedy. A second is affected with the same disease, and uses this remedy without any benefit; while a third recovers under a very different remedy, or without any treatment at all. And even in those cases in which he has distinctly ascertained true relations, new causes intervene and disappoint his endeavours to produce results by means of these relations. He knows, for example, a disease which would certainly be relieved by the full operation of diuretics;—and he knows various substances which have unquestionably diuretic virtues. But in a particular instance he may fail entirely in relieving the disease by the most assiduous use of these remedies; for the real and true tendencies of these bodies are interrupted by certain other causes in the constitution itself, which entirely eludes observation, and are in no degree under his control." P. 21.

The great object of all inquiry in medicine is to discover the causes of this uncertainty, to obviate them as far as may be practicable, and, in the formation of general practical rules, never to attempt to establish any principle in which these causes of uncertainty are likely to interfere.

If we reflect on the circumstances of the various vicissitudes of medicine, we find that its uncertainty and fallacy may be traced to several sources. One of the first is the erroneous views which have prevailed in physiology. It is one of the vulgar errors to imagine that this consists of a series of conjectures, opinions and hypotheses; and, indeed, from the peculiar circumstances under which the science has been investigated, it too often proves to consist of hypothetical opinions instead of facts. Nothing, however, can be more certain than this, that physiology, the object of which is to discover and unfold the actions, processes, and functions of animal bodies, is, strictly speaking, a science of facts. A single example will render this familiar. When the ancient anatomists asserted that the arteries were mere air-vessels, this was simply an opinion supported by no fact. When Galen proved that the arteries as well as the veins contained blood, this was to state a matter of fact which could be proved by inspection on the living body. When it is asserted that the phenomena of the nervous system depend on the presence of a peculiar fluid, this also is asserting a matter of opinion which cannot be demonstrated, and which, consequently, we are entitled to infer, has no existence save in the minds of the authors of the opinion. Digestion, to give another instance, has been supposed successively to depend on coction or boiling; on mechanical contraction or trituration; on chemical action or fermentation; and on solution by a peculiar fluid. The three first explanations, however, were mere matters of opinion, which could not be demonstrated to the satisfaction of any unbiassed inquirer. The fourth is that which, at least in the case of the membranous stomachs, has been proved to be by far the most probable mode of explaining the process. But it must be remarked, that the solution is accomplished under circumstances so peculiar, that it bears little or no analogy to mere chemical solution.

When the physiologist undertakes to explain the functions of the animal body, he may do so in two modes. Either he may take the assemblage of functions, and consider the individual processes of which they consist, the agents by which they are effected, and the modifications which they present in different classes of animals. Or he may undertake to investigate the uses of individual organs, and to determine the part which each performs in the accomplishment of different functions. The first may be regarded as the duty of the physiological inquirer properly so denominated. The second is rather the duty of the anatomist when he describes the shape, situation, and constituent parts of individual organs. In either way the student is encompassed by numerous sources of fallacy. In the first, in which he examines the assemblage of actions which contribute to the performance of a function, he must analyse this function, and distinguish it into its constituent processes. He must ascertain by what mechanism, and by the action of what organ, each process is performed. He must consider the organization of the organ, and show the peculiarities of structure by which the process is effected. In this task it is easy to see that numerous difficulties occur. Notwithstanding, for example, the numerous experiments performed to ascertain the nature and causes of the process of digestion, how little is positively understood regarding it? What, for example, is the exact influence of the bile and the pancreatic fluid? Could digestion not be performed without them? By what mechanism, or by what chemical process, is chyle formed? What are the circumstances essential to its production; and what is the change which it undergoes on being united to the mass of blood?

All these points, it is impossible to deny, are imperfectly understood. But no physiologist deserving the name ever thinks of stating, as ascertained facts regarding them, conjectures or hypotheses, unless as such. All of the processes *must* exist as actual facts in the living body, and may certainly be determined in some mode or other by sufficient patience and accuracy in observing, or by ingenuity of experimenting; and if conjectures are substituted in place of the facts so determined, that is not the fault of the science, but of those by whom it is cultivated. To rectify this inconvenience it is requisite to observe phenomena as they occur in the living body patiently, accurately, and repeatedly; where they are complex to analyse them; and where they are equivocal or ambiguous to place them in such situations by means of experiment, as may determine them in one mode or other. Even the phenomena of disease, by occasionally performing accidental experiments on the living body, and excluding successively the operation of adventitious or accessory agents, may place before the eyes of a vigilant observer the essential cause in actual operation. In other instances, by appealing to the varieties of structure exhibited in the animal world, the same or a similar result is obtained.

In modern times, the improved state of the collateral sciences has furnished the means of obtaining more accurate information on the true nature and object of the functions of the living body than either the ancients or even our immediate predecessors could ever hope to acquire. How much, for example, has the discoveries of the constitution of the atmosphere, and the knowledge of the numerous gases which modern chemistry has unfolded, contributed to rectify, to enlarge, and adjust our knowledge of the function of respiration? From the first experiments of Priestly and Lavoisier to the recent ones of Allen and Pepys, and Edwards, how much more precise has this department of physiological science become?

Another of the principal sources of retardation, nay, of retrogradation to this department of knowledge, must not be omitted. In many instances, in which the physiologist cannot trace phenomena to a palpable or demonstrable cause, he forthwith imagines or creates one to which he believes he may refer those phenomena. However convenient the terms of *sympathy*, as used by Whytt, and *excitement* and *collapse* as used by Cullen may appear, it is impossible to doubt that they have been not a little abused, and that they have been often employed merely to designate complex actions, the nature of which was totally unknown, and to which no precise or distinct idea was attached. The same may be remarked of the term *irritation*, and that of *congestion*, as employed by various authors of the present day. It is, nevertheless, worthy of notice, that these terms were by no means so vaguely or extensively used by their first authors as by their subsequent followers. The only mode of explaining this progressive abuse of terms, and the ideas attached to them, is found in the fact, that the pupils of men of genius are ever more prone to carry the principles of their teachers to a much greater degree of extravagance than the authors of these principles themselves. It is a well-known fact, that while Leibnitz was in the habit, in the society of his friends, of speaking in jest of his theory of *monads* and *pre-established harmony*, his pupil, Wolff, believed in them most seriously, and defended them on all occasions with infinitely more address and vigour than Leibnitz himself could have done. The disciples of Stahl, in like manner, in their zeal for the principles of their preceptor, carried them to a much greater length, and made much more bold applications of these principles, than Stahl himself even ventured to do,—an office for which they were only laughed at by the author. Even several of the pupils of Cullen, more distinguished for zeal and enthusiasm than discretion, defended with great spirit some of his least defensible hypotheses; and the system of Brown, which was better supported by Darwin than by its author, found still more energetic and laborious advocates in Germany and Italy. Most of the present generation can testify the energy with which the disciples of Broussais defend the principles of their master.

It was remarked by Bacon, that the study of final causes is barren and unproductive; and, in his strong and figurative language, he compared it to the vestal virgin, who, consecrated to the divinity, gives birth to no offspring. This proscription Mr Stewart regards as greatly too general, and very ingeniously shows, from the subsequent reasoning of Bacon himself, that he did not recommend its entire exclusion from the cultivation of science, but simply to be placed under strict limitation. Mr Stewart afterwards proceeds to show, that the investigation of final causes is improper neither in morals nor in physics, and that it has been productive of the greatest benefit in the scie of anatomy, evidently understanding by that, that species of physiology which we allow to be the second. The passage is so much deserving the attention of the physiological inquirer, that we must be allowed to give it in Mr Stewart's own words.

"Of these *facts*, the most remarkable are furnished by the science of anatomy. To understand the structure of an animal body, it is necessary not only to examine the *conformation* of the parts, but to consider their *functions*; or, in other words, to consider their *ends* and *uses*. Nor, indeed, does the most accurate knowledge of the former, till perfected by the discovery of the latter, afford satisfaction to an inquisitive and scientific mind. Every anatomist, accordingly, whatever his metaphysical creed may be, proceeds, in his researches, upon the maxim, that no organ exists without its appropriate destination; and although he may often fail in his attempts to ascertain what this destination is, he never carries his scepticism so far as for a moment to doubt of the general principle. I am inclined to think, that it is in this way the most important steps in physiology have been gained; the curiosity being constantly kept alive by some new problem in the animal machine; and, at the same time, checked in its wanderings, by an irresistible conviction that nothing is made in vain. The memorable account given by Mr Boyle of the circumstances which led to the discovery of the circulation of the blood, is but one of the many testimonies which might be quoted in confirmation of this opinion.

"'I remember, that when I asked our famous Harvey, in the only discourse I had with him, (which was but a little while before he died,) what were the things which induced him to think of a circulation of the blood? he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed, that they gave free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was invited to think that so provident a cause as nature had not placed so many valves without design; and no design seemed more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries, and return through the veins, whose valves did not oppose its course that way.'"*

This passage we have quoted chiefly for two purposes, to show that the term final cause ought to be banished from philosophy, and that Bacon was right in his limitation of the study of final causes. The term *final cause*, which appears at first sight a contradiction in terms, is known to be applied to the last, in that chain of events to which we can trace any arrangement either in the moral or material world. Viewed more closely, it implies the connection of the last effect produced with the first cause, and consequently, that, in the arrangement made, it was proposed or intended to produce the effect. It is on this account that the disquisition of final causes resolves itself into that of ends, objects, purposes, or uses, and that it would be best, at least in physical or physiological inquiries, to speak of *ultimate intentions*, the *objects* of certain arrangements, or the *purposes* to which they are subservient. It would lead us into a field much too extensive to illustrate these observations practically; but certain we are, that every one accustomed to reason on material or vital phenomena, and to trace the connection between them and the effects which they produce, will be able to furnish numerous apposite examples from his own experience.

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* Boyle's Works, Vol. iv. p. 539.—Folio Ed.

It is also to be observed, that the term final cause is objectionable, because it is used in two senses, or at least in a sense in which two distinct objects are confounded. By one class of philosophers it is used in the sense now mentioned, of intention, purpose, or use; and by another it is employed to denote the purpose in the mind of the Creator, with which the arrangement was made. However similar or identical these may be, it cannot be doubted that they are distinct; and the neglect of distinguishing them has given rise to much of the misapplication of the term.

If these observations be well founded, it will follow that the limitations to the study of final canses inculcated by Bacon were perfectly well founded. In no department of natural science, and in none of medical science, has the desire to discover final causes been productive of so much injury as in physiology. Let any one peruse the *Elementa* of Haller, and he will see there in the history of opinions and conjectures, how many singular, extravagant, and imaginary notions have been indulged in, in assigning uses to different organs. Let any one look into the work of Richerand, and he will see there many similar remnants of the conjectural physiology, from which even some of the best modern systems are not altogether free. Why was the brain at any time likened to a secreting gland? and why, at the present moment, is it compared to a galvanic pile or an electrical battery? What is to be said of the uses which Hewson assigned to the thymus gland and spleen? These, and similar examples, which shew how cautious we ought to be in our inquiries into the uses of organs, are the circumstances which have brought physiology into discredit, and which have made it be regarded not as a generalized assemblage of facts, as it ought ever to have been, but as the common receptacle for all the most extravagant fancies and foolish conjectures with which the seething brains of speculatists have teemed. To look at this science, any one would have said that it was that of which Tully, in prophetic spirit, had asserted, *nullum tam absurdum quod aliquis philosophorum non somniaverit*. In all our inquiries, therefore, especially regarding uses and purposes, we must never forget that in some instances it is impossible to discover any purpose whatever, and that the organ is there because it belongs to the general system of organic parts, more fully developed in other *genera*, and that in all it can only be discovered by observing the effects of its removal either by disease, by accident, or by experiment, and not by mere conjecture.

In one department of physiology, that of the human mind, particular caution is requisite; and in this the example has been so well set by Mr Stewart himself, that we think it can scarcely be expected to be improved. The greater part of the present volume which is devoted to this subject exhibits, we must say, a very clear, simple, and intelligible exposition of these views, illustrated by some forcible and interesting examples. Our limits do not allow us to enter into particulars; but we cannot do better than recommend the perusal of the present work along with that of Mr Stewart, for the purpose of communicating a complete history of the intellectual faculties, and the best mode of improving them.

The subject of insanity, which may be regarded as partly the pathological state of the mental and moral faculties, partly the effect of that of the brain, is considered after somnambulism under the aberration of reason. This is scarcely, according to our arrangement, the proper plan to introduce the author's views of the causes, nature, and varieties of mental derangement. But as we may not elsewhere have a more convenient opportunity, we shall notice them in the shortest possible manner.

Dr Abercrombie, regarding the peculiar character of insanity to be, that a certain impression fixes itself indelibly on the mind to the exclusion of all others, allows that we know nothing of the condition of the mental faculties on which it depends. We may nevertheless observe the facts; and these he attempts to refer to the following heads.

"I. Propensities of character, which had been kept under restraint by reason or by external circumstances, or old habits which had been subdued or restrained, developing themselves without control, and leading the mind into trains of fancies arising out of them. Thus a man of an aspiring ambitious character may imagine himself a king or great personage; while, in a man of a timid suspicious disposition, the mind may fix upon some supposed injury, or loss either of property or reputation.

"II. Old associations recalled into the mind, and mixed up perhaps with more recent occurrences, in the same manner as we often see in dreaming. A lady, mentioned by Dr Gooch, who became insane in consequence of an alarm from a house on fire in her neighbourhood, imagined that she was the Virgin Mary, and had a luminous halo round her head.

"III. Visions of the imagination which have formerly been indulged in, of that kind which we call waking dreams, or castle-building, recurring to the mind in this condition, and now believed to have a real existence. I have been able to trace this source of the hallucination. In one case, for example, it turned upon an office to which the individual imagined he had been appointed; and it was impossible to persuade him to the contrary, or even that the office was not vacant. He afterwards acknowledged that his fancy had at various times been fixed upon that appointment, though there were no circumstances that warranted him in entertaining any expectation of it. In a man mentioned by Dr Morison, the hallucination turned upon circumstances which had been mentioned when his fortune was told by a gipsy.

"IV. Bodily feelings giving rise to trains of associations in the same extravagant manner as in dreaming. A man mentioned by Dr Rush, imagined that he had a Caffre in his stomach, who had got into it at the Cape of Good Hope, and had occasioned him a constant uneasiness ever since. In such a case, it is probable, that there had been some fixed or frequent uneasy feeling at the stomach, and that, about the commencement of his complaint, he had been strongly impressed by some transaction in which a Caffre was concerned.

"V. There seems reason to believe that the hallucinations of the insane are often influenced by a certain sense of the new and singular state in which their mental powers really are, and a certain feeling, though confused and ill-defined, of the loss of that power over their mental processes, which they possessed when in health. To a feeling of this kind, I am disposed to refer the impression, so common among the insane, of being under the influence of some supernatural power. They sometimes represent it as the working of an evil spirit, and sometimes as witchcraft. Very often they describe it as a mysterious and undue influence which some individual has obtained over them; and this influence they often represent as being carried on by means of electricity, galvanism, or magnetism. This impression being once established of a mysterious agency, or a mysterious change in the state and feelings of the individual, various other incidental associations may be brought into connection with it, according as particular circumstances have made a deep impression on the mind. A man mentioned by Pinel, who had become insane during the French revolution, imagined that he had been guillotined,—that the judges had changed their mind after the sentence was executed, and had ordered his head to be put on again; and that the persons entrusted with this duty had made a mistake, and put a wrong head upon him. Another individual mentioned by Dr Conolly, imagined that he had been hanged, and brought to life by means of galvanism; and that the whole of his life had not been restored to him." Pp. 316–319.

Dr Abercrombie adopts the view of melancholy common to the authors of this island, without seeming to allow that it is one of the forms of *monomania*, or partial derangement. This he does on the ground of the circumstance, that the same patient is at one time in a state of melancholic depression, and in the other in a state of maniacal excitement. This, however, is true with regard to all the forms of partial insanity, in which it is quite common to observe the patients wrong on every topic, or decidedly maniacal. In this country we believe it is most common to observe the *monomania* of depression more frequently than that of ambition, love, or any other single emotion or passion. It is also to be remarked, that we more frequently observe the *monomania* of religious bigotry in this than in any other country, and not unfrequently spreading in an epidemic form.

The attempts which have been made to trace insanity to the state of the bodily organs, Dr Abercrombie thinks have not been attended with much success, chiefly because in a large proportion of cases it has been impossible to trace any connection of this kind. We know not whether, in the present state of pathological knowledge, this inference is perfectly warranted. For in the cases in which it has been impossible to trace any connection between mental derangement and disease of the material organs, it may always be doubted whether the inspection was sufficiently accurate; whether the observers were competent to determine what was a morbid change of the organs; or whether that attention was given to pathological subjects which they now receive. From the views and several of the facts recorded in the present work itself, especially those relating to somnambulism and spectral illusions, we think a large proportion of evidence might be adduced to show, that every form of mental derangement depends on more or less change in the state of the nervous system. The only instance which furnishes any thing like an exception is that of the home-sickness, (*nostalgia*,) which, especially in those interesting exiles, the Swiss, appears to commence at first in a mere moral affection, which eventually terminates in, if it does not cause, extensive changes in the corporeal organs. In most, if not all other, instances, the mental derangement appears to proceed from previous affection of some of the corporeal organs, most commonly the brain or its membranes or vessels. Nor is this hypothesis to be rejected on the ground of its favouring materialism. It no more favours materialism than the opinion that delirium depends on fever or accompanies *hysteria*; or, in other words, that the state of the immaterial faculties is influenced by that of the material organs.

Regarding the operation of moral causes as inducing insanity, we in general agree with the present author. At least, he will find that in a former article on this subject we took occasion to express an opinion which we have had good and sufficient reason to believe is the correct one, that the states of mind which are said to cause insanity are simply indications that the individual is already insane. An enthusiastic and rather fanatical young person, for example, conceives that the form of religion in which he has been trained by his parents is erroneous and imperfect, and that it is utterly impossible for him to be saved if he abides in his present opinions. He proceeds in a manner quite irregular and extravagant to make proselytes; and if he fails in this, to seclude himself from the world, committing at the same time various absurdities, which leave no doubt that he is deranged on this point. In such circumstances, little observation is requisite to show that religious peculiarities in this case were not the cause, but the effect of derangement, and that the individual was deranged before he proceeded to adopt peculiar opinions, and act in an extravagant manner.

Under the head of treatment by moral and other means, we find the following observations illustrative of a peculiar variety of mental derangement.

" A most interesting affection of this class often comes under the observation of the physician, consisting of deep but erroneous views of religion—generally accompanied with disturbed sleep, and considerable derangement of the system, and producing a state of mind closely bordering upon insanity. It occurs most commonly in young persons of acute and susceptible feelings, and requires the most delicate and cautious management. Two modes of treatment are frequently adopted in regard to it, both equally erroneous. The one consists in hurrying the individual into the distraction of company, or a rapid journey; the other, in urging religious discussions, and books of profound divinity. Both are equally injudicious, especially the latter; for every attempt to discuss the important subject, to which the distorted impression refers, only serves to fix the hallucination more deeply. The mode of treatment, which I have always found most beneficial, consists of regular exercise, with attention to the general health; and in enforcing a course of reading of a nature likely to fix the mind, and carry it forward in a connected train. Light reading or mere amusement will not answer the purpose. A regular course of history, as formerly mentioned, appears to succeed best, and fixing the attention by writing out the dates and leading events in the form of a table. When the mind has been thus gradually exercised for some time in a connected train of thought, it is often astonishing to observe how it will return to the subject which had formerly overpowered it, with a complete dissipation of former erroneous impressions. A common complaint at the commencement of such an exercise is, that the person finds it impossible to fix the attention, or to recollect the subject of even a few sentences: this is part of the disease, and, by perseverance, gradually disappears. This experiment I have had occasion to make many times, and it has always appeared to me one of extreme interest. I do not say that it has uniformly succeeded, for the affection frequently passes into confirmed insanity; but it has succeeded in a sufficient number of instances to give every encouragement for a careful repetition of it. The same observations, and the same mode of treatment, apply to the other forms of partial hallucination. The plan is, of course, to be assisted by regular exercise, and attention to the general health, which is usually much impaired. The affections are particularly connected, in a very intimate manner, with a disordered state of the stomach and bowels, and with derangements in the female constitution. Means adapted to these become, therefore, an essential part of the management." Pp. 338–340.

On the subjects of idiocy and fatuity, there are several interesting observations and facts, to which, however, our limits allow us merely to allude. Spectral illusions, Dr Abercrombie considers under the following heads. 1*st*, False perceptions or impressions made on the senses only, in which the mind does not participate; 2*d*, Real dreams, though the individual was not at the time sensible of having slept; 3*d*, Intense mental conceptions, so strongly impressed on the mind as for the moment to convey the belief of real existence; 4*th*, Erroneous impressions connected with bodily disease generally affecting the brain; and, 5*th*, Mere misconception, depending on excessive activity of the imagination operating on inaccurate sensible observation. The manner in which these different sources of spectral illusion take place, is ingeniously and in general justly explained. Several of them which the author distinguishes, probably belong to the same head, and differ merely in the different degrees on which the imagination operates on the process. All of these, doubtless, depend on the facility with which the faculty, assisted occasionally by the memory, recals past objects, or creates new ones totally independent of any impression made at the moment by the external objects by which the individual is surrounded. Those in which there is some morbid state of the brain or its circulation are probably the most common; and it is not unlikely that, if the circumstances connected with the others were accurately examined, these also would be found to be in a great measure referable to the same principle.

The second source of the uncertainty of medicine, to which we now return as more immediately connected with our present subject, consists in the difficulty of generalizing the phenomena.

of disease, of tracing them to uniform causes, and of thereby applying means for their alleviation or removal. This source of uncertainty to which Dr Abercrombie devotes the fourth part of his work, he refers to two general heads—the want of uniformity in the characters and progress of disease, and in the action of external agents on the body. As on this topic the author appears to have reflected much and judiciously, we think our readers will not thank us for attempting to state his views in any other than his own words; and we must therefore request their attention to the following observation on these two kinds of fallacy.

" I. There is great difficulty, in medicine, in tracing effects to their true causes, and causes to their true effects. This difficulty has already been illustrated, by the same cause appearing to produce in different instances different diseases, or no disease at all; and by a disease seeming to subside under the use of a remedy, which, in a similar case, fails to produce the smallest benefit. When we find our researches thus encumbered with uncertainty, we cannot, as in other sciences, clear them from the influence of casual relations, by means of direct experiment; but are obliged to trust chiefly to the slow course of observation, as the relations happen to be presented to us. Hence just conclusions are arrived at slowly, and we may be obliged to go on through a long course of observations, before we arrive at any results which we feel to be worthy of confidence. Hence also arises the great temptation to grasp at partial and premature conclusions, from which medical science has suffered so much injury. For, when such conclusions are brought forward with confidence, as long a course of observation may be required for exposing their fallacy, as might have been sufficient for ascertaining the truth. In this respect, we see the remarkable difference between medicine, and the purely physical sciences; as in the latter, a single experiment may often be sufficient to overturn the most plausible hypothesis, or to establish one which has been proposed only in conjecture.

" II. Even after we have ascertained the true relations and tendencies of things, we are constantly liable to disappointment in medicine, when we endeavour to produce certain results by bringing these tendencies into action. This arises from the silent operation of a new order of causes, by which the phenomena of disease are varied and modified; and by which the action of external agents is aided, modified, or counteracted, in a manner which altogether eludes our researches. The causes which thus operate are certain powers in the living body itself, the action of which is entirely beyond our control; and others arising out of the peculiarities of age, sex, temperament of body and mind, and mental emotions; constituting a class of agents of a most powerful kind, of which it is impossible to estimate the combined operation. It is farther to be kept in view, that these various agents may be acting together, or in opposition to each other, or under a variety of combinations; and that, in reference to our attempts to act upon the body by remedies, they may be operating in concert with, or in opposition to, these attempts. Hence arises a most extensive source of uncertainty in all our investigations, of which it is impossible to calculate the effect, or the extent. Hence also arises that apparent want of uniformity in the phenomena of disease, by which we are so much impeded in our researches; and that want of uniformity in the action of remedies, by which our efforts in medicine are so often disappointed.

"III. Another source of uncertainty in the practical art of medicine, is the difficulty which we find in applying to new cases, the knowledge which we have acquired from observation. This application is made, upon the principle either of experience or analogy. We are said to proceed upon experience, when the circumstances in the new case are the same, as in those cases from which our knowledge is derived. When the circumstances are not the same, but similar, we proceed upon analogy; and our confidence in the result is weaker than when we proceed upon experience. The more numerous the points of resemblance are, the greater is our confidence, because it approaches the more nearly to that which we derive from experience; and the fewer the points of resemblance, this confidence is more and more diminished. When, in the practice of medicine, we apply to new cases, the knowledge acquired from others which we believe to have been of the same nature, the difficulties are so great, that it is doubtful, whether in any case, we can properly be said to act upon experience, as we do in other departments of science. For we have not the means of determining with certainty, that the condition of the disease, the habit of the patient, and all the circumstances which enter into the character of the affection, are in any two cases precisely the same: and, if they differ in any one particular, we cannot be said to act from experience, but only from analogy. The difficulties and sources of uncertainty, which meet us at every stage of such investigations, are, in fact, so great and so numerous, that those who have had the most extensive opportunities of observation, will be the first to acknowledge, that our pretended experience must, in general, sink into analogy, and even our analogy too often into conjecture."—Pp. 371-374.

By what means, it must next be asked, can the medical observer entertain the rational hope of removing these evils and inconveniences. It is most likely to be accomplished, the author thinks, by attention to the following rules; 1*st*, To acquire an extensive collection of authentic facts; 2*d*, To classify them properly; 3*d*, To trace the relations which subsist among them, especially those of cause and effect; and finally to deduce general principles.

The necessity of these rules must be so obvious to any one who has reflected for a moment on the subject, that we conceive it quite superfluous to dwell on them at any length. The author makes, however, some judicious observations on the besetting sins of fact-collectors, from which we think it may not be altogether useless to give the following.

"IV. Receiving as facts, on which important conclusions are to be founded, circumstances which are trivial, incidental, or foreign to the subject. For example, in the investigation of affections of the spinal cord, appearances have been often considered as indicative of disease, which we have good reason to believe have arisen merely from the position of the body after death. In the same manner, in the investigation of a certain class of diseases, an important place has been assigned to slight appearances in the gastro-intestinal membrane, which, we have reason to believe, are entirely incidental, and worthy of no confidence in a pathological inquiry.

"V. Receiving as facts, statements which falsely obtain that name. The sources of fallacy to be kept in view under this head are chiefly the following.

"(1.) Receiving as facts, statements which are not facts but opinions.—A person dies after being affected with a certain set of symptoms, and we find, on examination after death, the usual appearances of hydrocephalus. Another is seized with similar symptoms, and recovers. He is therefore said to have recovered from hydrocephalus, and such a statement is often given as a medical fact. The man's recovery from certain symptoms is a fact; that he recovered from hydrocephalus is not a fact but an opinion.

"(2.) Receiving as a fact, a statement which only assumes the relation of facts.—A person recovers from a particular disease, while he was using a particular remedy. His recovery is ascribed to the effect of the remedy; and the cure of the disease by this remedy is often given as a medical fact. The man's recovery is a fact; and that he used the remedy is another fact; but the connexion of the remedy with his recovery we are not entitled to assume as a fact. It is tracing between the facts the relation of cause and effect,—a process of the utmost delicacy, and not to be admitted on any occasion, but with the greatest caution.

"(3.) Receiving as facts, general statements, or the generalization of facts. One of the most common examples of this error occurs, when a statement is given of a symptom or set of symptoms, as certainly diagnostic of any particular disease, or of a particular morbid condition of an internal organ. Such a statement we hold to be of no value, except we have absolute confidence in the narrator, both in regard to his habits as a philosophical observer, and to the extent of the observations on which his statement is founded. But, with every possible advantage in these respects, we are to exercise the utmost caution before we receive the relation, thus stated, as a fact; for it is to be kept in mind, that it is not properly a fact, but a generalization of facts. Some writers, for instance, have maintained with much confidence, that a particular state of rigidity of some of the limbs is distinctly characteristic of ramollissement of the brain. But farther observation has shown that the disease may exist without this symptom, and that this condition of the limbs may appear in connexion with other diseases. Their observation of facts was in so far correct, that this state of limbs does very often accompany ramollissement of the brain; the error consisted in giving it as a general fact, or a fact applicable to all cases of ramollissement—which is without foundation. Yet such statements, when brought forward with confidence, are often received as facts, and rested upon as established principles; and then the facts by which their fallacy might be detected are apt to be overlooked or forgotten.

" This may perhaps be considered as one of the most prevailing errors in the modern science of medicine; and it is indeed astonishing to observe the confidence with which such statements are brought forward, and the facility with which they are received as equivalent to facts, without attention to the manifold sources of fallacy with which they are encumbered. Does a writer, for example, tell us he has ascertained that the spinal cord is diseased in all cases of tetanus. If we knew that such a statement had been founded on the careful observation of an hundred cases, it would be of value; if it was deduced from a few, its value is greatly diminished. But even if it had been deduced from the larger number, certain doubts would still arise in considering the relation thus stated as a fact. We should naturally ask ourselves,—was the narrator qualified to judge of the facts and their relations,—were the cases referred to, all really cases of Tetanus,—were the appearances in the cord such as could properly be considered as indicating disease,—or might any of them have been mere changes of colour, or other incidental appearances, which might have taken place after death, or might have been the effect of the convulsion rather than its cause,—or were they such changes as may be found in other cases without any symptoms of Tetanus? Other sources of fallacy will come into view, if the statement be, that the narrator has uniformly found a certain remedy of great efficacy in a particular disease. Here, in the first place, similar questions occur as in the former instance;—on how many cases did he found his statement,—how did he ascertain the disease,—and was he qualified to decide that it really was a case of the disease which he alleges. But, supposing all these questions to be answered in a satisfactory manner, others still arise, namely,—had the alleged treatment really any influence on the recovery of the patients,—did they get well in consequence of the treatment, or in spite of it, or altogether independently of it,—have not similar cases recovered spontaneously, or under modes of treatment entirely different.—Such is the uncertainty of causation and generalization in medicine; and such is the danger of receiving general statements as equivalent to facts."—Pp. 378-382.

Dr Abercrombie then proceeds to speak of the necessity of arranging accurately those facts which have been verified, and properly cautions the student against the love of system, and especially that of nosological distinction. This passion has certainly been carried to an undue length in modern times; but we are satisfied that this, which is the abuse of classification, constitutes no legitimate objection against the use. A good and perfect nosology is the aim of all systematists, but the difficulties of observation and verification have hitherto operated as insurmountable impediments to its accomplishment; and the result is, that we have no perfect system, but only an approximation to it. It is evident particularly that where erroneous facts are admitted, as in the instances stated by the author, instead of benefit, they are productive of serious injury. Every system of nosology, therefore, must be received and used with caution and discretion; and, as the author justly observes, it must be our constant study by further observation to clear them successively of every source of error. The only test of accuracy is when we find that we can verify our systems, which are at best artificial helps, by a rigid comparison with actual cases.

The difficulty of connecting phenomena in medicine in the relation of cause and effect, is still more serious, and opposes perhaps obstacles still more insurmountable to actual perfection; and both in the progress of morbid actions, and in the employment of agents reputed remedial, we have daily and inexhaustible sources of this species of inconvenience. The following observations on the connection of events of this kind, may communicate some idea of the manner in which Dr Abercrombie investigates the subject.

"I. The connexion which we observe may be entirely accidental. That causation should be assigned on grounds so slight, as to admit of this explanation, may appear improbable; but no person acquainted with the history of medicine will find difficulty in pointing out examples of it, especially in the effects which are often ascribed to remedies on the slightest possible grounds. In this manner, by some bold and confident assertion, founded, probably, on very limited observation, a remedy is frequently brought into repute, as nearly infallible in a certain class of diseases, which we find, in a very short time, consigned to oblivion.

"II. The events may be closely connected, but not as cause and effect. They may be effects of a third event, which is the cause of both; or they may be parts in a sequence, in which we have still to discover the true antecedent. Thus, in the examination of the bodies of those who have died of hydrocephalus, the liver has frequently been found in a state of disease; and, upon this ground, diseased liver has been stated as one of the causes of hydrocephalus. This must be considered as an example of false causation, for, in its reference to hydrocephalus, diseased liver cannot be considered in any other light than as another effect of a common cause, —namely, as a result of that unhealthy state of the constitution in which hydrocephalus is most apt to occur. When the nature of croup was first investigated, the formation of a new membrane was observed in the larynx, and this was considered as the cause of croup. Farther investigation, however, showed that this was but one of a chain of sequences, the real antecedent of which is inflammation of the membrane lining the larynx.

" III. The events may be really connected as cause and effect, while there is difficulty in assigning to them their places in the sequence; that is, in determining which is cause and which is effect. This occurs when, in ascertaining the facts themselves, and the order of their sequence, we are obliged to trust to external indications of actions which are going on in internal parts. It is a difficulty of frequent occurrence; and want of due attention to it appears to have been the source of much fallacious reasoning. In the investigations, for example, respecting the nature of continued fever, various morbid appearances have been observed in internal parts; and each of these has, in its turn, been confidently assigned as the cause of fever. Thus, one has placed the seat and cause of fever in the brain; another in the spinal cord; a third in the gastro-intestinal membrane; and, according to one of the latest doctrines, inflammation of the mucous follicles of Peyer and Brunner, has been confidently stated as the cause of every modification of fever. It does not belong to this part of our inquiry to examine minutely the comparative merit of these systems. In all of them there is a correct observation of facts, and probably a real relation of cause and effect; the error consists in fixing the order of the sequence; for a very slight view of the subject is sufficient to show that the morbid appearances on which they rest must be considered as effects, or incidental concomitants of fever, not as its cause. This kind of false causation may also occur in other subjects. In certain states of the weather, for example, many people are in the habit of saying that a shower brings a change of the wind. There is every reason to believe that there is between the two events a real connexion of causation, but the meteorologist rather tells us, that the change of the wind produces the shower.

" IV. When we observe a particular change in the living body, and trace an apparent relation between it and some external agent which seems to be the immediate antecedent, it may very often happen that other agents are concerned which elude our observation;—though they have been the real antecedents or agents in the change which has taken place, or have contributed to it in a very great degree. This is a principle of most extensive application in medical causation, and is one of the chief causes of its difficulty and uncertainty. The agents referred to are chiefly certain powers in the living body itself. These in all cases exert a greater or less influence in the changes which are the objects of our inquiries, and, in many cases, are the sole agents in producing results which we falsely and ignorantly ascribe to our remedies. When a disease has terminated favourably, our treatment may have been proper, and may have co-operated with these powers;—it may have been totally inefficient and harmless, and have had no influence whatever in producing the result;—or it may have been improper and hurtful, and yet these powers may both have thrown off the disease, and have counteracted the effects of our blind interference. It is unnecessary to allude, also, to the extensive influence which, in certain classes of diseases, is produced by passions and affections of the mind, often of so delicate a nature, that even the person who is the subject of them shall not be aware of their influence."—Pp. 390-394.

The section on *deducing general principles* should be perused sedulously by all those who wish to form correct views of the proper mode of investigation in medical subjects. It may be doubted whether the author is altogether right in his general denunciation of the study of causes, and especially of efficient causes. The statement that the study of nature is the study of facts not of causes, is more antithetical than just. Any one who considers the matter for a little, will soon be satisfied, that, of a series of facts which require to be determined, several are, in the strict language of philosophy, the causes of the others; that these are certainly proper objects of investigation; and that even in the study of facts the principal object is to discover their causal connection. It is not indeed the class of causes distinguished as efficient which are beyond the reach of the human faculties, as Dr Abercrombie appears to imagine; but several of those denominated *final;* and it is to these especially that the term *philosophy of causes* was applied in the ancient philosophy. This class of causes, and the question how far they ought to become objects of investigation in philosophy, we have already attempted to consider. Efficient causes are of a very different nature; and, notwithstanding the criticism of Stewart, whom Dr Abercrombie evidently follows, while Bacon, Newton, Reid, and Adam Smith, are authorities on the opposite side, in attaching to them the signification of causal events, they must be allowed to be perfectly legitimate objects of inquiry. To take an example,—it is impossible to doubt, that, to determine whether the motion of the heart is the efficient cause of the motion of the blood, is a proper object of investigation; or that the admission of air to the lungs is the efficient cause of the change which the blood undergoes from venous to arterial. When we come, however, to inquire why the blood is removed from the heart to the extreme parts of the system, why it acquires there a dark colour, and why it is afterwards rendered bright red in the lungs, it is obvious that these, which are final causes, are less fit, though not less interesting, objects of inquiry.

The concluding observations on the use of hypothesis and theory are judicious, and merit the serious attention of many who do not know the distinction between these terms, and who attach to them no other meaning than that of objects to be execrated.

The volume is concluded by an able view of the qualities and acquirements which constitute a well regulated mind. These Dr Abercrombie refers to the following heads; the cultivation of habits of steady and continuous attention, careful regulation and control of the succession of thoughts, the cultivation of an active inquiring state of mind, the habit of correct association, careful selection of the subjects to which the mind ought to be directed, due regulation of the fancy, cultivation of calm and correct judgment, and lastly, a sound condition of the moral feelings. These subjects the author discusses with much judgment; and though on the last topic these remarks savour a little of the declamatory style, they are in a good vein, and show that the author feels an interest in the cultivation of those elevated moral qualities which communicate the feelings of moral responsibility, and without which the brightest and most highly cultivated talents lose half their lustre and most of their utility. Though we do not adopt all the views of the author regarding the absolutely necessary connection of moral and intellectual qualities, for this would be to deny the evidence of daily observation, we cannot help often wishing that it were the case; and certain we are, that the principles which Dr Abercrombie maintains on this head cannot fail to be of much use to the rising generation, whether professional or unprofessional.

ART. V.—1. *On the Tendency to Calculous Diseases, and on the Concretions to which such Diseases give rise.* By JOHN YELLOLY, M. D., F. R. S., &c. (From the Philosophical Transactions.) London, 1830.

2. *A Further Inquiry into the Comparative Infrequency of Calculous Diseases among Sea-Faring People, with some Observations on their frequency in Scotland.* By ALEXANDER COPLAND HUTCHISON, F. R. S. L. and E., Surgeon to the Westminster General Dispensary, and a Vice-President of this Society. (From the 16th Vol. of the Medico-Chirurgical Transactions of London.) London, 1830.

THE first of these papers is the concluding part of that which, on a former occasion, we brought under the notice of the readers of this Journal. Like that, it consists of two parts, a chemical and statistical; and as the latter accords in subject with that of Mr Hutchison, we shall follow the order of our former analysis in adverting to it first.

The statistical division of both papers relates chiefly to the prevalence of calculous diseases in Scotland. Dr Yelloly has, after considerable pains taken in the acquisition of information on this point, given the following results:—

"The operations which have been performed at the Dundee Hospital, on cases belonging to Dundee, and to the county of Forfar generally, are about 54 in 36 years, or 1.38 per annum; this, as the population is 113,000, is at the rate of one case for 107,000 inhabitants.

"In the Aberdeen Hospital, the proportion is much more considerable; for in the course of 77 years, as by a list which Mr Cromar of that establishment was so good as to transmit to me, 285 operations have occurred, on cases belong to Aberdeenshire, and to the town of Aberdeen, containing together, a population of 155,000; which is at the rate of one case for every 42,000 inhabitants. On the other hand, in the Hospital of Inverness, not more than 5 operations, as Mr Hutchison states, have occurred in the last 20 years, which, for the population of 90,000 contained in Inverness-shire, including the town of Inverness, is not at the rate of more than one for every 300,000 inhabitants. But when it is considered, that this hospital is resorted to by the poor of the contiguous counties of Ross and Nairn, as well as by those of Cromarty and Sutherland, the extreme paucity of stone cases in this mountainous district must be manifest.—In the Infirmary of Glasgow, 49 cases have occurred, in the course of 15 years, according to a list which Dr Macfarlane did me the favour to forward to me, or about 3.26 per annum; and of these, 31 belonged to the city and suburbs, containing about 147,000 inhabitants, which is at the rate of 2 per annum, or 1 for every 71,000 inhabitants. The remaining 18 were from the country; but I am unable to state the districts from which they were derived. There is reason, however, to suppose, that the tendency of the neighbouring counties to this disease, is very much less than that of Glasgow, for they have only afforded as small a number of calculous diseases as 18 to the Infirmary of that city, which may be regarded as the principal establishment for cases requiring capital operations in the west of Scotland.

"In the neighbouring town of Paisley, consisting of 38,000 inhabitants, about 18 cases have occurred in 10 years, all of them of poor inhabitants of the town, which is at the rate of 1.8 per annum, or one for every 21,000 inhabitants.

"In the Infirmary of Edinburgh, 41 cases have occurred in the last 10 years; but I have not been able to procure, separately, the numbers which were derived from the city and the country respectively. Taking, however, the proportions as similar to what is found to be the case in the Glasgow Hospital, (which is probably not far from the truth,) there would be 24 of that number belonging to the city of Edinburgh, including Leith, and containing a population of 138,000, which would be at the rate of 2.4 per annum, or one case for every 57,000 inhabitants.

"In the southern and south-west parts of Scotland, as well as in the northern, the disease is exceedingly rare; for Dr Craigie of Edinburgh informs me, that is is hardly known in the Dumfries Hospital, which, as being the only establishment of this kind south of Edinburgh and Glasgow, takes in a very large district in that part of the kingdom; and Sir George Ballingall, the Regius Professor of Military Surgery in the University of Edinburgh, states to me, on the most respectable authority, that in Kelso and its neighbouring district, calculous complaints scarcely ever occur.

"The county of Northumberland, and that part of the county of Durham which is north of the Tyne, a good deal resemble, in the unfrequency of calculous diseases, the adjoining districts of Scotland; for by a list which was obligingly transmitted to me by Dr Headlam of Newcastle, it appears, that 95 cases of stone operation occurred in the Newcastle Infirmary during the last 30 years, which is at the rate of 3.6 per annum. Of this number, 64 belonged to the above district, including Newcastle, with the addition of Gateshead, which lies on the opposite bank of the Tyne, in the county of Durham; and these afforded 2.13 cases per annum, which, as the population was 213,000, gave one case for every 100,000 inhabitants. But if the country district be taken without Newcastle or Gateshead, there will then be 29 cases in 30 years for a population of 166,000, and one case for every 172,000 inhabitants."—Pp. 421-422.

Almost nothing has hitherto been done to determine the lithophorous tendency of Ireland; and the first observations of this nature are given by the present author. It appears from the inquiries made by Dr Yelloly, that stone is comparatively a rare disease in Ireland, that in some districts it is extremely unfrequent, and that in others it is altogether unknown. In the counties of Antrim, Armagh, Londonderry, Donegal, Fermanagh, Tyrone, Carlow, Kildare, Kilkenny, and Longford; in King's County; and in those of Lowth, Wicklow, Clare, Kerry, Galway, Roscommon, Tipperary, and Mayo, containing together a population of above 3,500,000, not a single operation of lithotomy has occurred in any of the respective hospitals since their establishment; nor has a single example of the disease among the lower orders of these districts come within the knowledge of any of the professional gentlemen who have answered the inquiries of Dr Yelloly.

"In the counties of Down, Monaghan, Leitrim, Sligo, Limerick, and Waterford, and in Queen's County, the population of which amounts together to about 1,200,000 persons, 9 cases of stone operation only have occurred, during the whole time to which the records of the hospitals, or the information or inquiries of their medical officers extend, and which embrace a period hardly short of 40 years. This is at the rate of not more than 0.25 per annum, or one case in 4 years.

"In the city and county of Cork, containing together above 800,000 inhabitants, about 13 operations of lithotomy have been performed in the last 18 years, or about 0.66 per annum, of which 10 occurred to Dr Woodroffe of the South Hospital.

"In the hospitals of Dublin, including the Meath or county of Dublin Hospital, it appears from information with which I have been favoured by Mr Roney, late President of the Royal College of Surgeons of Dublin, and Mr Crampton, the Surgeon-general of Ireland, that about 6 cases occur in the course of the year; and this estimate is confirmed by Mr Carmichael's opinion, as stated by Mr Hutchison.

"Making a suitable allowance, therefore, for those counties from which I have been disappointed in not yet receiving returns, it does not appear that more than 8 operations of lithotomy occur annually among the poor of Ireland, the whole population of which was considered, in 1821, to be about seven millions. But when we refer 5 of those cases to the city and county of Dublin, containing together a population of about 350,000, (which, from the documents with which I have been favoured, seems to very near the truth,) it will be seen how minute the proportion of calculous cases must be in the remaining population of Ireland, when not more than 3 operations of lithotomy occur annually among the poor of a population of between six and seven millions."—P. 23, 24.

The inquiries into which this investigation has led Dr Yelloly, tend still to confirm the general conclusion which he formerly stated, that calculous diseases are much more frequent in the town than in the country. These results are stated so distinctly and briefly in the language of the author himself, that we cannot hope to present them in a more condensed form by abridgement.

"In the town of Newcastle and Gateshead, calculous cases have occurred, within the last 30 years, in the proportion of about one for every 40,000 inhabitants; while in the county of Northumberland, independently, together with that part of the county of Durham which borders on the Tweed, the proportion, (as I have already remarked,) has only been one for 172,000 inhabitants.—In the town of Nottingham, the proportion, in 48 years, has been at the rate of one for every 67,000 inhabitants; while in the county of Nottingham only, it has been one for every 146,000 inhabitants.

"In the town of Dundee, the calculous operations have occurred in the proportion of one to 41,000 inhabitants; yet in the county of Forfar, in which Dundee is situated, the proportion has been one to 82,000.

"In Glasgow, the proportion has been, in the last 15 years, about one in 71,000; in Paisley, in 10 years, one in 21,000; and in Edinburgh, in a similar time, one in 57,000; while there is every reason for supposing, (though I cannot speak from direct evidence upon the subject,) that the stone cases afforded to any of those establishments, by country population, is exceedingly small. It is to be remarked, however, that the proportional numbers furnished by the town and country population of Aberdeenshire, during a period of 70 years, are nearly alike; or rather, that those of the country population have somewhat preponderated; for while Aberdeen is afforded to the amount of one case for 44,000 inhabitants, the proportion of Aberdeenshire alone, without any obvious cause for such difference, has been one for 40,000 inhabitants.

" In Ireland, the great preponderance of calculous cases originating in towns, is likewise strongly marked; for in Dublin, to judge from the returns of the county hospital for the last 12 years, two-thirds of the cases, or 4 per annum, seem to be furnished by that city, which is about one-half of the number afforded by the whole remaining part of Ireland, and is at the rate of one for every 45,000 inhabitants. In Cork likewise, the proportions are much less than those of Dublin, being about one for 160,000 inhabitants; yet this infinitely exceeds the usual product, either of the county of Cork, or of Ireland generally.

" Where the circumstances which have a tendency to produce calculous diseases are so very obscure, so difficultly traceable, and so full of anomalies, I have thought it useful to notice the local situations which are remarkable either for the frequency or unfrequency of such maladies, because the attention of observers may thus be directed to analogies, or discrepancies, which may not before have been sufficiently the subject of remark. That there are certain affections of the digestive organs which favour the occurrence of calculous complaints, is very generally admitted; and that these affections are likewise exceedingly prevalent in towns, and more especially among persons who practise sedentary occupations, is likewise well known. But while dyspeptic complaints are so common, that they form a very large share of the diseases which present themselves in the medical charities, both of London and the country, it still remains a problem, to what particular circumstances of constitution or habit, the origin of stone is to be attributed, which, in comparison of other chronic maladies, is so rare; and what are the peculiarities which render some dyspeptics liable to the complaint, and thousands of others exempt from it.

" That there is a certain connection between calculous tendency, and some prevailing diathesis of towns, is supported by the greater frequency of stone in town, than country populations; and I was inclined to consider it not improbable, that this diathesis might be the scrofulous, both from mesenteric diseases prevailing so much among the children of crowded cities, and from scrofulous affections being very common in Norfolk. Still, however, the same difficulty presents itself, as occurs with regard to affections of the digestive organs; namely, why so minute a portion of scrofulous persons should be affected with this disease, and so large a number escape. I am disposed, indeed, to consider the idea of a probable connection between a scrofulous and calculous diathesis, as very much weakened by the consideration, that in many of those parts of Ireland, where calculous complaints are hardly at all known, scrofulous affections are stated to me to be exceedingly common.

" I must still confess the difficulty of referring the disposition to calculous affections, to any known circumstances of air, water, soil, or habits of life. The eastern position of Norfolk and Suffolk, produces more than the average bleakness of other parts of the kingdom, particularly in the spring months; but this inclemency is exceeded, both in the northern parts of the island, and the more elevated parts of Ireland. The poorer part of the population have much of a farinaceous aliment, often not well fermented; but still the main article of food is fine wheaten bread, which has been, for a long period, in such common use over the whole of England. In Scotland, too, the latter is, as I am informed, fast superseding the bread made of the coarser grains.

" In Ireland, the diet of the humbler classes of society is very much confined to potatoes, and butter-milk, or skim-milk; often, unfortunately, however, without either of the latter; and it appears highly probable that this diet, and perhaps, to a certain extent, the use of ardent spirits, is unfavourable to the formation or deposition of lithic acid, with which the tendency to calculous diseases is much connected, The freedom from this formidable class of complaints seems, however, to be dearly purchased, by the want of many of the comforts which the labourers of England and Scotland possess; nor is it accompanied by a less than usual tendency to many of the most ordinary complaints of Great Britain, as pulmonary consumption and scrofula, which are mentioned by several of my intelligent correspondents, as being very frequent among the Irish commonalty. The effect of a town residence, in producing a tendency to calculous complaints, is very strongly exemplified in Dublin, where they are so much more prevalent than in other parts of Ireland. This circumstance will, I venture to hope, meet with the particular attention of the Dublin faculty, who unite, in so high a degree, the requisites for prosecuting a difficult professional investigation."—P. 424-427.

The paper of Mr Hutchison, which is a further prosecution of his former researches on this subject, demonstrates the accuracy of the general conclusion stated by that gentleman, that calculous diseases are rare among persons attached to the seafaring life. From inquiries made at the principal sea-ports of Great Britain, he has obtained results which are digested in the following table.

TABLE showing the total number of CASES OF STONE Operated upon in a given Period, at the under-mentioned SEA-PORT Towns, distinguishing their Age, Sex, and such as were in any manner allied to a SEA-FARING LIFE, as far as could be ascertained.

Places.	No. of Years.	No. of Cases from correct Register.	No. of Cases from incorrect Register.	Total.	Average No. of Cases annually, (nearly.)	Age.		Sex.		Sea-faring Persons.		
						Under 14 Years.	Above 14 Years.	Males.	Females.	Sailor Boys.	Seamen.	Fishermen. Pilot.
ABERDEEN Infirmary, . .	10	68	—	68	$6\frac{8}{10}$	13	55	63	5	—	—	2
Amsterdam, (Chief Hospital) .	1	2	—	2	—	—	—	—	—	—	—	—
Bristol Infirmary, . . .	10	33	—	33	$3\frac{3}{10}$	15	18	31	2	—	—	1
Dublin, ...	1	—	6	6	6	—	—	—	—	—	—	—
Dundee Infirmary, and Arbroath,	37	78	37	115	$3\frac{4}{37}$	21	57	75	3	—	—	—
Edinburgh, and Leith, . .	10	63	12	75	$7\frac{5}{10}$	15	48	63	—	1	—	1
Glasgow and Greenock Infirmaries,	12	32	—	32	$2\frac{8}{10}$	9	23	32	—	—	—	—
Gerenwich Hosp. for decayed seamen,	10	—	—	—	—	—	—	—	—	—	—	—
Guy's Hospital, (London) .	7	—	44	44	$6\frac{2}{7}$	—	—	—	—	—	—	1
Haslar, Naval Hospital, . .	13	2	—	2	—	—	2	2	—	—	—	—
Hull Infirmary, (Yorkshire) .	25	—	62	62	$2\frac{1}{2}$	—	1	1	—	—	1	—
Liverpool Infirmary, . .	13	—	16	16	$1\frac{3}{13}$	8	8	14	2	—	—	—
London Hospital, (London) .	10	41	—	41	$4\frac{1}{10}$	19	22	36	5	—	—	—
Newcastle-upon-Tyne Infirmary, &c. *	10	31	—	31	$3\frac{1}{10}$	—	31	31	—	—	—	—
Norfolk and Norwich Hospital, .	11	122	—	122	11	43	74	117	5	—	—	—
Plymouth Naval Hospital, .	13	2	—	2	—	—	2	2	—	—	1†	—
St Petersburgh Imperial Hospital,	14	32	—	32	$2\frac{4}{14}$	—	—	—	—	—	—	—
St Thomas's Hospital, (London.)	10	—	77	77	$7\frac{7}{10}$	—	—	—	—	—	—	—
		506	254	760	—	143	341	467	22	1	2	5

* This includes Sunderland.

† This is the Cotton Case mentioned at p. 13, in a note.

From this it appears, that of the number of 182.463, the annual average of those employed in sea-faring occupations, there are, in the period of 14 years, only 7 persons who become the subjects of operation for stone, or about one in 364.926 persons.

From the observations and inquiries of Dr Yelloly, calculous diseases are not less uncommon among the military. During the last 15 years, only 4 cases of *calculus* have occurred in the British army, and 2 in the division in Ireland. In France, it appears from the communications of Baron Larrey, that in the course of 30 years, only 5 operations of lithotomy have been performed at the Gros Caillou; 4 on soldiers, and 1 on a soldier's child; and 1 operation at the Val de Grace. According to the report given by M. Gama, for 6 years, during which he was chief surgeon to the military hospital of the latter institution, and 8 previous, while he held the same office in the military hospital of Strasbourg, he has not once had occasion to perform the operation of lithotomy. The disease is, in short, very rare among soldiers.

In the present paper, also, Dr Yelloly gives the result of the analysis of the residue of the Norwich hospital collection of concretions, amounting to about 333 specimens. As the general results are not materially different from those exhibited by the table in the former paper of Dr Yelloly, which we gave at large, it is unnecessary to adduce the present one. We shall merely mention, that among the total number, 5 calculi consist of 1 substance, 17 of 2 and 4, and 26 of 3, showing that the greater number of urinary concretions consist of three deposits; that those consisting of lithic acid and lithate of ammonia, or their *nuclei*, are infinitely the most common; and that carbonate of lime, with phosphate of lime, and the mixed phosphates, constitute the leading characters of the entire collection.

In one specimen only *silica* was detected. It occurred in the form of minute crystalline colourless granules, diffused, or rather disseminated, irregularly through a dark brown calculus of oxalate of lime, so small as 5 grains in weight. Their extreme hardness being sufficient to scratch glass and agate, with their refractory character under the ordinary reagents, led Dr Yelloly to suspect them to be silicious; and on analyzing them conjunctly with Mr Faraday, gelatinized silica was obtained.

There are only 3 instances, Dr Yelloly observes, of silica in urinary *calculi* recorded, and in two of these the substance occurred, as in the present case, in combination with oxalate of lime; in the third along with phosphate of lime and lithic acid. The occurrence of silicious gravel is also mentioned by Dr Venables, of Chelmsford, (Quarterly Journal of Science, October 1829). The cause of its deposition, it may be imagined, is quite unknown.

PART III.

MEDICAL INTELLIGENCE.

CHEMISTRY.

Determination of the proportion of Fecula contained in various roots and fruits of the West Indies, with an analysis of the fruit of the Bread-fruit Tree.—Dr Ricord Madianna has communicated to the *Journal de Pharmacie* an account of the proportion of amylaceous matter which he has found by experiment may be procured from various common roots and fruits in the West Indies; and the following is the general result:—A pound of the root of the *Maranta arundinacea* yields two ounces and six grains of pure fecula. The same quantity of the *Canna coccinea* yields two ounces of equally pure fecula. A pound of the *Canna Indica* gives only four drachms and twenty-four grains of a grayish fecula, which may be whitened by washing, but is still much inferior to the two former. A pound of the *Canna lutea* yields only two drachms of an inferior fecula. A pound of the root of the *Amomum zingiber*, or common ginger, gives two ounces and sixty grains of a yellowish, pungent, fecula, which by washing becomes white and equal in quality to arrow-root. A pound of the root of the *Amomum curcuma* gives no less than four ounces and eight grains of a yellow fecula, which by washing does not entirely lose its yellow colour, but is in every other respect equal to arrow-root. A pound of the root of the *Jatropha manihot* gives nearly two ounces and three drachms of an inferior fecula chiefly used for starch. The *Jatropha glandulosa* and *J. curcas* gave but a small quantity. A pound of the *Convolvulus batatas* yields nine drachms and twenty-six grains of very superior fecula. A pound of the root of the *Dioscorea triloba* gave four ounces of fine fecula; the same quantity of the *D. alata* three ounces six grains; of the *D. sativa* two ounces; of the *D. aculeata* one ounce and seven drachms; of the *D. piperifolia* three ounces; and a pound of the bulbs of the *D. bulbifera* gave seven drachms forty-four grains. A pound of the root of the *Caladium esculentum* gives four ounces of excellent fecula. A pound of the root of the *Arum peltatum* yields six drachms and thirty-six grains of an inferior fecula; and several other species examined by the author were still less productive. The seed of the *Mamea Americana* yields four ounces and ten grains per pound of a superior fecula: which, however, must be carefully separated from a poisonous principle also contained in the seeds.

The fruit of the *Artocarpus incisa*, or Bread-fruit Tree, contains two ounces, one drachm, and fifty grains of fecula. But in addition it contains a considerable proportion of other principles not less important in relation to its nourishing properties. It contains three drachms and twenty-five grains of vegetable albumen; and nearly three ounces of gluten with vegetable fibre. The other component parts of this fruit are eighty grains of a vegetable soap, ten grains of resin, and a large proportion of water.

MEDICINE AND THERAPEUTICS.

Moscow, 30th Nov. 12th Dec. 1830.

The Cholera in Moscow in a Letter from a Correspondent there.—The news of the disease having shown itself on the Russian frontier, bordering on Persia, last year, attracted little attention. When, however, we were informed that the disease, after ravaging Tabris, the seat of the Persian government, had spread to Teflis, the capital of Georgia, and thence to Orenburgh, and its government, Astracan, and, following the course of the Volga, leaving the colony of Sarepta untouched, (who are said to have saved themselves by a strict observance of orders, cutting off all communication without the city,) had got to Saratoff, we began to think that it might visit us, which, as I have already informed you in my last, proved but too true. The disease showed itself unequivocally, though sporadically, from the 12–24th to the 15–27th September last, and is pretty generally believed to have been imported from Saratoff, or the fair of Nishney Novogorod, though this is a subject which has not yet been sufficiently investigated, but shall be, if God spare me, as soon as I can command leisure. From this time it gradually, but not slowly, increased, showed a strongly-marked epidemic character, particularly affecting the lower classes of people, the ill-fed, ill-clothed, living in low and damp houses or cellars, and whose habits of intemperance, and carelessness of health predisposed them to the action of the efficient cause, whatever that may be. About the 14–26th October there were, by the public accounts, 1066 sick of cholera; and on that day 244 were seized with the disease. It seems then to have been at its acme, and from that time has gradually declined. The number taken ill two or three days ago amounts to sixteen or seventeen daily; in consequence of this diminution, several of the public temporary hospitals have been shut up, and several others, of which that I have had the charge of is one, have not received patients for these last ten or twelve days. On the 5–17th November, the public accounts stood thus. From the appearance of the complaint till that day, were affected with cholera 6531, recovered 1813, died 3508, remained sick 1210. The total number affected with the disease by the latest statement, December 7-19th, amounts to 7000, of whom more than 4000 died. About the time the complaint appeared in Moscow, the government thought it necessary to intercept all direct communication with the town; and no one was allowed to leave the city, or come into it without observing quarantine. The working classes, to a considerable amount, were sent off to their villages, and many of the nobility left the city altogether, so that the population was considerably reduced.

The city is divided into twenty divisions, in each of which a temporary hospital was provided as quickly as might be; a medical council was formed, which met daily, and frequently twice a-day, at the General Governor's residence; and its members was formed principally from the inspectors of the hospitals, each of whom had under his charge or inspection several young surgeons and students. By the orders of the minister of the interior, about sixty medical men were drawn from Moscow for the service of the interior before the complaint began, so that the inspectors had all the responsibility, and a great deal of the anxiety and fatigue on their shoulders; and among others I was one, since all the young men that assisted me had seen but very little practice, and, of course, were but little fitted to take charge of such cases, and such a disease, as we had to do with. There were, in consequence, constant and strong calls on us, both in the way of duty and humanity. In every quarter there is a police station, where some medical men are in attendance; and strict orders were issued, that, so soon as any one was taken ill in the quarter, information should be immediately sent to the police station, whence a medical man was immediately sent to examine, and make his report; When symptoms of cholera appeared, he either immediately prescribed, or bled the patient, or, what we afterwards, in our quarter at least, found better, dispatched him in a carriage with four horses, which stood yoked day and night, and conveyed him straight to the hospital. In this way as little time was lost as possible. From one cause or other, however, partly the ignorance and indifference of the lower classes, partly their dislike to the hospital, and carriage, &c. we never got patients to the hospital of which I had charge sooner than six hours after the commencement of the disease, seldom so few as eight, and most frequently ten, twelve, eighteen, or more. In this manner two patients never entered the hospital, but died in the carriage that brought them, eleven before any medical assistance could be given them, and a great many in three hours, sometimes more, sometimes less. After they were brought to us, several died in the warm bath, one of the first remedies we used; indeed, a very great number were in such a state, as to be out of the reach of medicine, being half dead, and without pulse at the wrist.

Under these and other disadvantages, such as the great press of business, the distance of the hospital from my house and from the apothecary's shop, and the time lost between the prescription and the exhibition of the medicine, it is no wonder, that in so rapid and formidable a disease as the Indian cholera, which it undoubtedly is, I should only have been able to save 56 out of 164, the number brought to us dead, dying, and very ill. We had no slight cases, though a *few* where the disease was less distinctly marked; and some where, the characteristic symptoms being quieted, the complaint appeared under the form of continued fever, with biliary irritation and derangement in the secretion of the bile.

I had studied what the Indian writers had written on cholera, as far as my library afforded me the means, and, through the Edinburgh Medical and Surgical Journal, knew their practice; but my notions on the disease were chiefly drawn from my own experience in bilious cholera, and the close study for the greatest part of my life of the diseases of the digestive organs. I adopted the pathological views of this disease given by Dr Ayre in his practical work on the disorders called bilious, which were confirmed by the perusal of that part of Dr James Johnson's work on diseases of tropical climates relating to this disease, which I received about this time from England by Mr Harvey. I determined, however, to act with caution, as having to do with a formidable foe, and to judge for myself unbiassed by any one, and trusting to my own knowledge and practical experience; and as bleeding had been so much recommended by the Indian writers, I thought it right to try it in three cases, all of whom died. Two were strong men. One, however, though naturally strong, enjoyed but indifferent health; the other two were bled under unfavourable circumstances, so that all the conclusion I could draw from these was, that, under such or similar circumstances, bleeding was likely to do more harm than good; and as at this period of the epidemic the surgeons at the police were in the habit of bleeding almost every one they were sent to, I found it necessary to put a stop to such indiscriminate practice. Local bleeding I frequently found useful, and general bleeding, in one case particularly, was of great use in the course of this disease, where it had the appearance of abdominal inflammation; in another, a bleeding, employed early in the disease, and done effectually, that is, fourteen or sixteen ounces taken from the arm, followed by magnesia and rhubarb, cut short the complaint, which the patient, one of the young men of the establishment, had temporized with by using opium and oil of peppermint. The third day after the bleeding, he was at his duty again, and has remained well since.

Founding my practice on the pathological views which I have alluded to, I used a modified Indian practice, seeing clearly that unless I could make some strong impression on my patients at the first, the exhibition of medicine was in vain. A *medicina expectans* in such a state of things is worse than trifling; and having frequently experienced the good effects of twenty-grain doses of calomel in high excitement of the frame, I had no fear of giving such a dose to those who had no pulse at the wrist, so that my invariable practice was at first, till we got a vapour-bath fitted up, to strip the patient so soon as he was brought to the hospital, and put him, cold and shrunk, into the warm-bath, of which the temperature was gradually increased as high as he could well bear. The bath was made more stimulating, by adding a large proportion of common salt. While the bath was getting ready, we administered a glass of good brandy-punch; and when in the bath, watching a favourable moment when we thought the stomach was least likely to reject it, gave him twenty grains of calomel, mixed with as much gum-arabic, and washed it down as the Indians do, by from ten to twenty drops of oil of peppermint, mixed with an ounce and a-half of pure water, and, in general, forty drops of laudanum of the Edinburgh Pharmacopœia, or, instead of this, the calomel was washed down by a table-spoonful of Cogniac, and forty drops of laudanum, according to circumstances. This, or a diminished dose, was repeated, according to circumstances, a few hours afterwards. I rarely had occasion to give a third dose, as by the time we would have given it, the patient either showed morbid symptoms, or an abatement of the more urgent ones. It was then that I employed, what I consider a very essential part of the treatment of this disease, the regular and free, but prudent, use of purgatives, so as to follow up and assist the effects which I suppose to have been produced on the digestive organs by the calomel already given for this purpose. I have sometimes given an emulsion, with castor-oil and laudanum; but having found it often returned by vomiting, I generally used an infusion of senna, with manna and Epsom salts, unless where I feared inflammation; and in such cases I preferred the emulsion with castor-oil; in other cases these, though taken in free doses, were not sufficient to open the gall-ducts, so that I was forced to use something more stimulating, such as pills of rhubarb with calomel, or the compound extract of colocynth of Reuss's Pharmacopœia with calomel. By the use of one or other of these medicines, I commonly succeeded in procuring a free flow of morbid bile from the liver and gall-bladder, which I believed, and found by experience, to be the surest key to unlock the disease; for I lost no patient (excepting one where an infusion of senna was given injudiciously, and by mistake) in whom I succeeded in procuring a free biliary discharge by stool once. The watery stools and vomiting subdued, they never returned, unless the patient had a relapse by his own imprudence in eating. One of our young students, who entered on his office in bad health, was seized with the disease. By active treatment and great care, I left him in bed at the hospital at ten at night out of danger, after having been two days very ill. The next morning, on visiting him, I found him without pulse. On inquiry as to the cause of this sudden, and hitherto inexperienced, change, I was informed that, feeling himself disposed to eat soon after I left him the preceding evening, he had eat freely of some soup, and a fowl of which it was made. The quantity he took was more than his stomach could bear; brought on vomiting and purging, and such a collapse of the vital powers as defied all medicine. He sunk in spite of all my efforts to save him. This is not a solitary case. I am making preparations to try the effects of galvanism, electricity, and the protoxide of azote, or a considerable proportion of oxygen with atmospheric air. It appears to me probable that the primary impression of the poison which produces the disease is made through the organs of respiration on the blood itself, which appears to me to be unduly carbonized, and hence unfitted for the natural excitement of the nervous and muscular organs. Hence the morbid symptoms of diminished irritability of the heart, and undue influence of the brain on the muscles in general, as well as the torpor of the senses.

The *cordon sanative* has been removed thirty versts from Moscow.

Dec. 7-19th. By the last accounts the number affected with cholera exceeds 7000, and the number of deaths above 4000.

On the sanative effects of the Inhalation of Chlorine in Consumption. (*Archives Générales de Médecine, Novembre* 1830.)—Since the analysis of the treatises of M. Gannal, Sir C. Scudamore, and others, on the inhalation of chlorine and iodine in consumption was introduced into the review department of this Journal, a very important paper on the subject by *M. Cottereau* has come under our notice, of which we shall here give an abstract. The paper contains the details of thirteen cases of apparent cure; in most of which the existence of tubercular phthisis with tubercular excavations was proved, not only by the general symptoms, but likewise by stethoscopic indications; and in one of them the healing up of a cavity was proved some time after the cure was accomplished by the examination of the body, after death from continued fever. We shall present an abstract of each case, leaving out of view the general symptoms, which were as marked in all of them as appears necessary to convince every one that the essential features of phthisis were present, and passing over likewise the details of the treatment, as to which *M. Cottereau* advances nothing new; and we shall give merely a sketch of the symptoms before and after cure elicited by percussion and the stethoscope, as being the only evidence of the true nature of the disease which can be admitted to decide the question at issue.

Case I. is the same with that which has been noticed in page 387, from the treatise of *M. Gannal.*

Case II. A delicate female, wife of a physician. Towards the end of April 1828, the respiratory murmur was found to be wholly wanting in the summit of the right lung, where, both before, behind, and in the axilla, the sound of the voice was intermediate between bronchophony and pectoriloquy. In the left side there was dullness on percussion over the upper two-thirds. Both sides presented very generally mucous râle. On the 1st of June there remained only slight dullness of sound on the left side, and slight bronchophony with mucous râle on the right. After this no stethoscopic examination was made; but the lady soon got well, and continued so in November 1830.

Case III. An officer's lady, both of whose parents died of phthisis. In the middle of June 1828 there was crackling râle, sometimes dry, sometimes moist, in the lower region of the left lung, dullness on percussion, cavernous râle and feeble pectoriloquy in the upper part of the same lung, and sonorous râle in its middle posterior region. About the middle of August the dullness of sound on percussion was less, and the sonorous râle behind much diminished; but the other stethoscopic signs were unaltered. In the middle of September, there was no longer either crackling or cavernous râle, and the pectoriloquy had become doubtful; and at the end of the month there was neither pectoriloquy nor dullness on percussion. At the close of October she was free of complaint.

Case IV. A young Spanish gentleman, whose mother, as well as two of her children, died of phthisis, presented on the middle of July 1828, along with the general symptoms of the disease, the following auscultatory signs:—under the left clavicle a slight cavernous rale, with doubtful pectoriloquy, and dullness on percussion around this spot to the distance of two inches on every side. In the middle of October there was no longer any pectoriloquy or cavernous râle, and the dullness of sound on percussion was much more limited. At the end of that month the sound on percussion was every where natural, and the natural respiratory murmur was heard in every part of the lung. About a month afterwards he had a return of the general pectoral symptoms, which had previously ceased altogether, but there was no return of the phthisical indications supplied by the stethoscope on percussion, and he again got quite well in the course of December. From that period he has continued free of complaint.

Case V. A merchant, 35 years old, of spare habit, and liable to symptoms of impending phthisis for fifteen years, presented towards the end of August 1828, the following symptoms:—Dullness of percussion in the whole posterior surface of the left side of the chest, also remarked anteriorly though much less distinct, except under the clavicle; in which, last quarter, there was pectoriloquy, cavernous râle, and cavernous respiration in a very circumscribed point. At the end of September the cavernous râle had ceased, and the other signs, though still present, were less marked. About the middle of October there was neither pectoriloquy, cavernous respiration, nor even mucous râle, but merely dullness on percussion over the place where the indications of a cavity formerly existed. At this time the patient was in good health, and he continued well at the close of the ensuing winter.

Case VI. A medical student, 20 years of age, and of delicate constitution. In the middle of January 1828 there was generally obscurity of sound, and in some points complete dullness on percussion; anteriorly, near the middle of the left side of the chest, at a point corresponding with the seat of the pain, there was very distinct pectoriloquy, cavernous respiration and cavernous râle; and posteriorly there was general mucous râle. About the middle of April the mucous râle, cavernous râle, cavernous respiration and pectoriloquy, together with the surrounding dullness of percussion, had completely disappeared, and natural respiratory râle could be heard in every part of both sides of the chest. He was now in a state of full convalescence, soon regained strength and flesh, and at the time of the publication of the author's paper, continued to enjoy good health. This patient was examined stethoscopically by several physicians of note, both before and after the indications of a cavity ceased to present themselves.

Case VII. A bootmaker, 29 years of age, was examined by M. Cottereau in April 1829. Percussion gave some obscurity of sound over the whole right side of the chest, complete dullness an inch and a-half below the clavicle, and pretty natural sound in the left side, except at its inferior and superior regions; the stethoscope indicated a feeble natural râle at the lower part only of the right side,—in the axilla, under the clavicle, and in the supra spinous fossa of the scapula very distinct pectoriloquy, with mucous râle,—and in the left side natural râle everywhere except at the summit of the lung, where it was inaudible, and at the mamma where it was crepitating. At the end of May the general symptoms were greatly mitigated, but the auscultatory signs were the same. In the middle of June the obscure sounds of the left and right side on percussion were no longer remarked, the completely dull point of the right side was somewhat sonorous, the pectoriloquy doubtful, and the mucous râle scarcely to be heard. In the beginning of July the pectoriloquy had ceased altogether, and his general health was completely restored. This patient, too, was examined by several eminent physicians, who were convinced of the existence and subsequent disappearance of a cavity. In November 1830 he continued in a state of perfect health.

Case VIII. A tinsmith, 30 years of age, whose father died of phthisis, was examined for the first time by *M. Cottereau* in the middle of February 1829. The right side of the chest sounded obscurely over its lower two-thirds, and completely dull over its upper third, where the stethoscope indicated cavernous râle, and pectoriloquy both before, behind, and in the axilla; there was also general mucous râle of that side; in the left side percussion gave some dullness of sound under the clavicle, but elsewhere a clear natural sound,—and at the apex of the corresponding lung there was tracheal respiration. At the end of June the respiratory râle was natural in the lower third of the right side, while the cavernous râle there, as well as the general mucous râle, was scarcely to be heard; but the dull sound on percussion and pectoriloquy continued in the right side. In the middle of August there was no pectoriloquy, but still dullness of sound; and from having been apparently in an advanced state of phthisis he was able to resume his employment, had no cough, expectorated merely a little white mucus in the morning, sweated when he worked hard, and could not walk quick without having beating of the heart and dyspnœa. In August 1830 he was in the same state.

Case IX. A female, 28 years of age, whose mother and sister had died of phthisis, was attacked with the usual symptoms of that disease subsequently to a miscarriage. In the beginning of March 1829 she had complete dullness of sound at the summit of both lungs, and in the middle third of the left side, with much general mucous râle, and over a small point, two finger-breadths below the right clavicle, distinct cavernous râle and pectoriloquy. In the beginning of May M. Cottereau confirmed the opinion of her ordinary medical attendant as to the presence of these symptoms. On the 1st of August the only unnatural auscultatory sign which could be elicited was circumscribed dullness on percussion over that point only where the pectoriloquy was formerly heard. From that time till November 1830 she has continued quite well and free from any stethoscopic indication of a return of her complaint.

Case X. A merchant, 46 years of age, subject, as well as several other members of his family, to strumous enlargement of the glands, came under *M. Cottereau's* care with phthisical symptoms about the beginning of July 1829. The chest was more contracted on the right than on the left side; the left sounded well, but the right was dull in its two upper thirds; the left presented mucous râle above, and no unnatural stethoscopic indication elsewhere, but the whole right lung was almost impermeable to the air, while at the third intercostal space, two inches from the sternum, there was cavernous respiration and bronchophony approaching to pectoriloquy. On the 1st of October these signs no longer existed any where, the sound on percussion, the sound of the voice, and the respiratory râle being every where natural; and from that time he has continued in a state of perfect health.

Case XI. A lady 23 years of age, who had lost a sister from phthisis, consulted *M. Cottereau* for similar complaints in the beginning of July 1829. There was dullness on percussion under the right clavicle at the distance of three inches, tracheal respiration and distinct pectoriloquy at the same point and in the corresponding region behind, and in the left side mucous rale here and there without any other indication of disease. At the end of the following April percussion and the stethoscope no longer indicated any trace of disease, and her health was perfect, and continued so up to the date of the publication of the case.

Case XII.—This case, which is by much the most important of the whole, we shall relate more fully than the rest. A married female, 27 years of age, of delicate constitution, subject in her younger years to scrofula, and who had lost a sister from phthisis, was attacked in December 1827 with dry cough, gradually, but very slowly increasing in frequency. In April 1828 she had frequent hæmoptysis. Nine months after that she had a safe delivery, after which the pectoral symptoms increased in severity. In July 1829, when M. Cottereau first saw her, she was greatly emaciated, the skin had a dry leaden aspect, the eyes were dull, the extremities frequently cold, the appetite indifferent, her cough not severe but frequent, especially in the morning, with copious easy expectoration of greenish yellow or grayish, opaque, consistent masses, amidst clear, viscous mucosity; there was also constant pain in the larynx and between the shoulders, with shifting pains throughout the chest,—great oppression,—and almost complete loss of voice. The sound on percussion was very clear in a circumscribed spot about an inch under the right clavicle; around this spot perfectly dull, and throughout the rest of the right side somewhat obscure; but on the left side it was natural, except that some obscurity of sound existed in the lower third. In the axilla, as well as in the right subclavicular region, at the spot already mentioned, there was unequivocal cavernous respiration, cavernous râle and pectoriloquy,—around this spot a complete absence of respiratory murmur, and in the rest of the right lung tracheal respiration with slight crepitating râle,—in the left side natural respiration in the two upper thirds, mucous râle in the lower third. The pulse was 80, but very irritable under the slightest exercise, the breathing always hurried, and a hectic paroxysm occurred every evening, with its usual accompaniments and termination. On the 20th July the inhalation of chlorine was begun, but after a day or two an intermission was necessary, on account of an uneasy sense of heat and dryness in the back of the throat, which appears a very common effect of the chlorine when first used. In a few days, however, it was resumed, and gradually increased to ten inhalations per day. During the three first weeks of August two other intermissions were required on account of an increase of pain and a sense of heat in the chest. About the middle of September the general symptoms were greatly mitigated. For the ensuing month the inhalation could not be persevered in, on account of the state of the chest; but it was resumed at the middle of October, and continued regularly afterwards. At the middle of November the amendment was so great that the lady considered herself quite cured. Towards the close of December her state was as follows:—The appetite, strength, and flesh natural; the cough and expectoration gone; the breathing not affected by walking or climbing; the sound on percussion dull over a circumscribed spot, corresponding with the point where pectoriloquy was formerly heard, and the respiratory râle inaudible in the same quarter; but every where else the lungs appeared perfectly in their natural condition, free of morbid râle, as well as of pectoriloquy. The inhalation was persevered in for security's sake till the middle of January.—For three months afterwards this patient continued to enjoy uninterrupted health. Subsequently, however, she was exposed to frequent fatigue and night watching, in consequence of the illness of her infant; and at the end of August she was seized with symptoms of general fever, which proved fatal in four weeks, without having ever been accompanied with any signs of an affection of the lungs. In giving an account of the dissection, we may confine ourselves to the appearances in the respiratory organs. The epiglottis, larynx, and windpipe were natural. Two cervical glands on the right side were enlarged to the size of small nuts, and contained each a nucleus of friable, chalky-like matter. Both lungs were pale-gray, pliant, and crepitating. The left adhered here and there to the costal pleura by old adhesions. In the middle part of its upper lobe there was a tubercle as big as a pea, similar to the nuclei of the diseased cervical glands, and also some minute tubercles in the upper portion of the same lobe. The right lung was free of adhesion. "At the fore-part of its apex there was, over a space 18 lines long and 8 deep, a darker tint of the tissue, with very firm consistence, and an appearance of wrinkling; and when this mass was cut into, it was found composed of hard, compact, almost fibrous tissue, of a slate colour marbled with grayish-white; it was not traversed by any bronchial tubes, all of which were obliterated as they approached it. On the edge of this apparent cicatrix there was a small steatomatous-like tubercle, scarcely a line in diameter; and in various parts of the upper lobe of the same lung about a dozen minute miliary tubercles; but the tissue of that lung, both around the tubercles and in its other lobes, was quite healthy.

Case XIII. A corset-maker, 19 years of age, liable to scrofula, and whose father died of phthisis, was subjected to *M. Cottereau's* treatment in the middle of December 1829, whilst labouring under the usual general symptoms of consumption. Percussion produced in the left side, immediately below the clavicle, a clear sound,—around this point distinct dullness of sound as low as the inferior third,—and on the right side a dull sound in the upper fourth. The stethoscope indicated pectoriloquy in the upper third of the left side, both before, behind, and in the axilla, impermeability of the middle third, weak respiratory and subcrepitating râle in the lower third,—and in the right side impermeability of the upper third, natural or mucous râle in the remaining regions. At the beginning of August the dullness on percussion, the pectoriloquy and mucous and subcrepitating râles had entirely ceased, and with them her pectoral complaints. In November 1830 she continued to enjoy a state of confirmed good health.

The author concludes his cases with a short and moderately expressed paragraph, the close of which we recommend to the notice of Sir C. Scudamore and his associates, whose experiments on the same subject have been noticed in our review department. "Whatever opinion practitioners may form of these cases, they ought, at all events, to be induced by them to make personal trial of the inhalation of chlorine before passing final sentence as to its efficacy or inutility; and this is my only object in writing the present essay. A second series of cases, which will be published presently, will farther serve to show, that where the success of the remedy has been less complete, or actually nothing, the patients at least experienced obvious relief, and apparently a prolongation of their existence beyond what could be rationally expected in ordinary circumstances, considering the extent of the organic disease."

Account by Dr Wenzl of the Mineral Waters of the Island of Ischia. (Medizinisch-Chirurgische Zeitung, 2d September 1830.) The celebrated mineral waters and baths of the Island of Ischia near Naples, though resorted to annually by a crowd of visitors, have attracted very little attention of late years from medical men; and their nature and composition, as well as their properties, are by no means so generally known as they deserve to be. An interesting article on the subject, from which we extract the following particulars, has just been published by *Dr Wenzl* of Munich, containing the result of his observations during a late visit to the island, together with a copious analysis of a work published on its springs in 1822 by Professor Giudice of Naples.* Travellers have exhausted their fancy in describing the beauties and delicious climate of Ischia; and our author is determined not to be behind his predecessors, for he terms it "the pearl of the loveliest bay in the Old World." The force of the Libeccio and Sirocco winds is tempered by the lofty ridge of Epomeo, which rises about 3000 feet above the level of the sea, and crosses the island from east to west. Owing to this circumstance, together with the proximity of the sea, the extreme clearness of its sky is remarkable even in Italy. Hot springs are so abundant, that in many quarters of the island the soil cannot be dug to the depth of two feet without water rising from it, of the temperature of 99° and more.

The most noted spring is the Gurgitello, at a little village called Casamiccia, which owes its origin to the spring. The public baths are far from conveying any luxurious ideas to the visitor; for, as Dr Wenzl facetiously observes, "they are constructed neither of wood, nor of metal, nor yet of marble, but are simply *graves*, or holes dug in the floor of the bath-house, from seven to eight feet long, and two feet deep, lined with party-coloured tiles, in a kind of Mosaic work;" and the water is admitted, hot or cold, by two pipes, which are shut, not by stop-cocks, but with a *cork*. Various accounts have been given of the temperature of the Gurgitello. All agree, however, in stating, that the temperature rises about 45 degrees of Fahrenheit, between the beginning of spring and middle of summer; and the general belief is, that it ranges about 122° at the former, and 167° at the latter period. According to the analysis of Professor Giudice, a pound (7200 grains) contains 12 grains, or 25¾ cubic inches of free carbonic acid, 15.9 grains of muriate of soda, 11.7 sulphate of soda, 16.9 carbonate of soda, 3.6 carbonate of magnesia, 3.8 carbonate of lime, 1.0 ferruginous alumina, and 0.4 silica. The carbonic acid is retained at the temperature of 122°, but half of it escapes at 144°, and all of it at 167°. Some singular prejudices prevail among the Neapolitan physicians as to the use of this spring. Among these may be particularized, the notion that it should never be used for bathing before the month of July, because in the previous months it renders the body liable to colds. The first bath should not exceed ten minutes in duration, and even the last ought not to be continued, it is maintained, above half an hour. For this rule there may be an excellent reason in the circumstance that the surface of the water must be covered with a large quantity of carbonic acid gas, the long-continued inhalation of which may occasion disagreeable head symptoms: At the same time, not many years ago, it was the custom to keep the patient in the bath for two or even three hours together, and, so far as appears, without detriment; and, on the whole, it is a more rational rule, according to Dr Wenzl, to continue each bath, as in the case of warm-bathing generally, according to the patient's feelings of comfort, and not to observe any arbitrary rule. The ordinary duration of a course of warm-bathing includes twenty baths. Dr Wenzl recommends that eight or nine days be spent on the island after the course is finished; the climate, he says, being as superior to that of the adjoining quarter of Italy, as Italy is superior in the same respect to Germany. There is excellent accommodation in the hotels of the principal villages; and all the hotels maintain private baths for their inmates, to which the spring-water is brought in barrels.

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* Viaggio medico istituto, ad oggetto di riconoscere ed analizzare le acque minerale e le stufe.

Besides warm-baths, there is also at the Gurgitello an establishment of vapour-baths, consisting of sixteen baths in the form of niches, into each of which the natural vapour of the spring is conveyed by seven pipes. The temperature is about 88° F. This water is also taken sometimes internally in the dose of two cupfulls in the morning, with or without goat's milk, —chiefly in diseases of the lungs and bladder.

There is a munificent charitable institution at the village of Casamiccia, immediately opposite the public bath-rooms of the Gurgitello. This is a branch of a great charitable establishment at Naples,—an hospital for no less than 300 patients, who are transported thither from the capital, and maintained during the bathing months of July and August. In the hospital there is an immense hall, with eighty baths for the use of the inmates.

Next to the Gurgitello, the most celebrated spring of Ischia is the Acqua di san Restituta, which is at no great distance from the former, and close on the sea-shore. Its temperature is 84° when not disturbed, but when previously agitated it is 126°. Its solid contents are 68 grains in the pound of 7200 grains; and the contents are, hydrochlorate of lime 3 grains, hydrochlorate of magnesia 4.8, hydrochlorate of soda 29.4, sulphate of soda 25.1, sub-borate of soda 2, ferruginous alumina 2.3, silica 0.3. This spring enjoys much celebrity in diseases of the uterine system, such as fluor albus, polypus of the womb, incipient ulceration of the womb or vagina, tendency to scirrhus, impotence, and tendency to the formation of moles. Somewhat similar in composition and virtues to the San Restituta is the Acqua di San Montano, near the foot of Monte Vico, a principal eminence of the Epomeo range.

The purest of the mineral springs are two situated close to one another, the Fontana and Fornello, which agree in composition, and contain the same salts as the Gurgitello, but in much less proportion. Their saline contents, which amount to 26 grains only in 7200, are, carbonic acid 2, muriate of soda 8.3, sulphate of soda 3.8, carbonate of soda 6.6, carbonate of lime 2, iron 0.4, carbonate of magnesia 2.4, silica 2, and a gelatinous (vegetable?) matter 0.2. This water is used by persons in health, and also for some days by patients for whom the Gurgitello is considered too powerful to be commenced with abruptly. The Acqua di Citara is a purely saline spring, containing 30 parts of saline matter in 7200, chiefly composed of sulphates and muriates of lime, magnesia, and soda, with a considerable proportion of a soda alum. The spring of Olmitello enjoys considerable reputation in gravellish diseases. Its temperature at the source is 117°. It contains 18 grains of solid matter in 7200, of which 3.7 are muriate of magnesia, 3.5 muriate of soda, 7.6 sulphate of soda, 3.6 sulphate of potassa, and 0.3 silica. This composition gives certainly no insight into the source of its alleged virtues.—Besides these, several other springs are briefly mentioned, such as the Acqua del Occhio, which resembles the Gurgitello, and is usually united by beginners with the water of that source,—the Acqua delle Petrelle, the temperature of which is 212°,—and the Acqua Piscierella, which is remarkable as being the only cool saline spring in the neighbourhood of Casamiccia.

Besides the natural vapour-baths of the Gurgitello, several interesting baths of a similar description are to be found throughout the island. One of the most remarkable of these is the Stufa di Castiglione, the temperature of which varies from 133° to 140°, and which consists of watery vapour, carbonate of lime, and a little carbonic acid. Another of equal interest is the Stufa di San Lorenzo, which discharges with watery vapour a considerable quantity of carbonic acid gas, and sulphur also in the gaseous form. These vapour-baths are used in gout, rheumatism, anchylosis, paralysis, incipient dropsy, and inveterate diseases of the skin. Allied to these, may also be mentioned the Stufa di Testaccio, near the Olmitello spring. The emanations of this natural stove or fountain of heat differ from the rest in being extremely dry, so that even wet cloths suspended in them are speedily deprived of their moisture. The temperature is 111°. The nature of the hot emanations has not been determined.

SURGERY.

A Case of Severe Scald treated by the Nitrate of Silver. The following case of severe scald, by an anonymous Correspondent, demonstrates the utility of the nitrate of silver in this kind of affection, while it exemplifies its powers in changing and controlling the action of the capillary vessels.

A little child, 5 years old, was pushed backwards by another child, whilst naked, into a large pan of scalding water which had been just taken off the fire. It was taken out as quick as possible, and yeast was applied upon the injured parts. It was visited one hour after the accident. The whole of the back as high as the shoulder-blades, and as low as the middle of the thighs, was found severely scalded, the cuticle removed from some parts, and in other parts raised into large vesications. The whole of the belly, the penis, scrotum, and thighs, were also in a similar state, but not so severely scalded as the back. An opiate was administered, and the yeast was removed with a sponge and warm water; it was well that no oily application had been used, as its removal would have required more trouble and have given more pain. The loose cuticle was removed with that of all the larger vesications, and the small ones were punctured, so that a clear surface to which the nitrate of silver might be applied.

The whole surface was then moistened with pure water, and a long stick of the nitrate of silver was applied flat, once over the whole surface, and a little on the surrounding healthy skin. A little linen just moistened was then past over every part to diffuse the nitrate of silver, so that no spot might be left untouched. The child cried much less than was expected when the nitrate of silver was applied on the denuded cutis. The back on which the child would have to lie was then covered with neutral ointment spread upon linen, secured by a bandage. The thighs and belly were left exposed to the air to form an adherent eschar, being defended by a fracture cradle.

On visiting the child about eight hours afterwards, it was reported to have fallen asleep in a quarter of an hour after the application of the nitrate of silver, and to have complained of no pain since. There appeared no constitutional disturbance.

The very first morning after the accident this little patient was turned on his side enjoying some play-things with several play-fellows who were by the side of his bed. One part on the side of the thigh was much swollen and inflamed. It was discovered that the nitrate of silver had not been applied upon it. The whole of the belly and the other parts of the thighs exposed to the air looked very well, with scarcely any vesications; the eschars were removed in two places where the tapes of the bandage had crossed the belly; these parts were now defended by means of a small plaster of neutral ointment spread on linen. On some parts the eschars were floating on the serum, these afterwards became adherent. The scrotum and penis were much swollen, but gave no pain. The nitrate of silver was applied on the part not attended to at the first dressing.

On the second day the child was going on well; some of the eschars were becoming adherent; the scrotum and penis continued much swollen, but there was scarcely any pain, and that on the belly. There was slight heat of skin, and the tongue was a little loaded. A purgative with senna and salts was given.

On the third day nearly the whole of the eschars were found to be adherent, and the scrotum and penis less swelled.

On the fourth day the eschars were quite adherent on the belly, and the penis and scrotum were of their natural size.

On the fifth day the plasters of neutral ointment were removed from the back, which presented an appearance of a recently blistered surface, in a healing state, with some loose cuticle partially attached; there was no appearance of suppuration.

In several days more the back was healed, except in two or three small parts, which were scalded more deeply than the rest, and were covered over with coagulable lymph, nor the least suppuration having taken place. The eschars were peeling off the belly, leaving the subjacent surface quite healed.

On the tenth day this little patient was out of doors, and on the twelfth at school, every part being quite healed.

Mr Higginbottom, in his Essay on the use of the Nitrate of Silver, makes the following observations on burns and scalds:

"I have found that, by slightly passing the nitrate of silver once over a burnt surface, the pain is increased for a short time, but then totally subsides, vesication appearing to be prevented; the black cuticle peals off in a few days, leaving the part well. In cases in which the cuticle has been removed, the nitrate of silver applied on the surface induces an adherent eschar, and prevents the consequent ulceration."—P. 149.

"I have not had an opportunity of using the nitrate of silver in very extensive recent burns, but I can have no doubt of the benefit that would accrue from it. It should, I think, be applied over the whole surface of the burn or scald, once only but as in external inflammation; then the parts most severely burnt should be covered with lint, and the whole of the burnt surface with the neutral ointment spread on linen, a bandage being applied to retain the dressings in their places. I should expect that the inflammation would be checked, and the consequent vesication, ulceration, and sloughing, in a great measure prevented, except in those places where the fire had actually destroyed the parts deeply. I should not examine the parts again before the fourth or fifth day; and if the dressing adhered I would let them remain during another similar period. The application of the nitrate of silver should be repeated in the same manner, as might appear to be required. I think the burn would then be limited in its extent, and would consequently be less dangerous; for the danger is generally in proportion to the extent of surface destroyed. The nitrate of silver has certainly the property of removing the irritability of the whole surface to which it is applied, and cannot add much to the pain of the burn itself."—P. 150.

These anticipations appear to be correct when the nitrate of silver is applied to a burnt or scalded surface from which the cuticle is not removed. It has the immediate effect of subduing the heat or burning pain, preventing vesication, and causing it to terminate by resolution.

When the skin is denuded of the cuticle and the nitrate of silver is applied, this most irritable and inflamed surface is converted into an insensible covering, which remains adherent until the inflammation is gone, and the new cuticle is formed underneath, at which period it loosens and drops off.

The application of the nitrate of silver is equally efficacious whether the burned or scalded surface be afterwards exposed to the air or covered by the neutral ointment. In the first case an adherent eschar is formed in two or three days; and in the second the effects of the nitrate of silver appear to continue for four or five days, producing a constant flow of serum, which continues until all the inflammation, irritation, and pain, are gone. It is possibly of little consequence which plan is adopted, as both are healed about the same period. The adherent eschar would be preferable in parts exposed, as the face and neck; or the chest, belly, or legs too, if defended by a fracture cradle, and the patient in bed.

The advantages of the nitrate of silver in the treatment of burns and scalds appear to be of the very first importance. We have at once a covering for the injured and very irritable surface superior to any other formed and composed partly of the very surface itself. The nitrate of silver acts as an *anti-inflammatory* agency both immediately and for several days after its application.

It may be safely applied over the head, chest, or abdomen, and it is not, like arsenic, and some other remedies used externally, liable to be absorbed into the system.

Spontaneous Luxation of the upper cervical vertebra. (Archives Générales de Médecine Novembre et Decembre 1830*).*—The two following examples of spontaneous subluxation of the occiput on the atlas, and of the atlas on the second vertebra of the neck, have been related, the one by M. Ollivier, the other by M. Martin Solon, in the French Archives.

1. A girl seven years of age was attacked in January 1824 with pain and swelling of the right knee, for which linseed poultices were ordered, and which, under this simple treatment, made no progress for a twelve-

month. Subsequently, however, an abscess formed above the internal condyle of the thigh-bone and made its way outwards, discharging at first a large quantity of cheesy matter, and afterwards a serous-like fluid. In the course of a few months, four other abscesses formed in various quarters of the knee-joint and neighbouring part of the calf, which burst, put on the appearance of scrofulous sores, and, together with the first, did not heal up for two years. At this time she came under the care of *M. Ollivier*, who found the inner condyle of the thigh-bone swelled and tender, and two fistulous openings discharging serum above the ham. Under the use first of cataplasms and then of mercurial friction, with a diachylon bandage, the joint became greatly better and the fistulous openings healed up; but at the same time a glandular swelling appeared in the right groin, which suppurated and burst. This abscess healed up quickly, and in three months the patient was nearly free of complaint.—Towards the close of December 1829, she began to complain of pain in the back of the neck, difficulty of moving the head, and swelling of the cervical glands on each side. Some relief was obtained by the application of leeches and poultices; but the sense of stiffness on moving the head continued. In three weeks the pain in the neck returned; it was deep-seated and dull, with occasional acute pricking pain, and was increased on motion, so that the child kept the head constantly in a state of complete immobility. As the cervical glands had again swelled and become painful, *M. Ollivier* appears to have been led to consider their diseased state the cause of the symptoms; and therefore he resorted again to leeches, poultices, and opiate and oily frictions, followed by mercurial inunction. For a month the disease made no apparent progress. But at length, towards the end of April, swallowing became somewhat impeded, though no disease could be detected in the throat; a uniform swelling of the back part of the neck had filled up the whole depression there; slight pressure occasioned sudden increase of pain, and at times a quick lancinating pain along the course of the posterior branches of the first cervical pair of nerves; the countenance had a peculiar expression of anxiety; the head was drawn a little backwards; and when she was desired to look at an object at her side, she turned the eyes without moving the face. There could now be no doubt of the existence of disease in the joint of some of the vertebræ of the neck, and most probably in the atlanto-occipital joint. Under the use of leeches and mercurial frictions, the disease rather gained than lost ground. The pain extended over the back part of the head, and especially behind the left mastoid process; the patient felt as if cold water were constantly running under the skin in that quarter; the head was thrown farther back and inclined to the right side, and the girl supported it behind with her hands, when she walked or rose from the recumbent posture. These symptoms became more and more distinct during May and June, deglutition then became much more difficult, pressure on the nape of the neck caused a sense of numbness in the arms, the lower jaw was protruded so as to project two lines before the upper; and the mouth could be opened but to a short distance, and not without pain. About the middle of June the head, which for two months had been much inclined backwards and to the right, began to be gradually drawn to the left side; the symptoms of compression of the spinal cord became more distinct,—deglutition being more difficult, the arms, more especially the right, somewhat feeble and often numb, and the eyes also feeble and easily fatigued. A caustic issue was now applied to the nucha. In fifteen days there was a sensible amendment, the pain becoming less, the headach not so violent, the appetite better, and sleep less disturbed. In the beginning of August the patient, who had been confined five weeks to bed, began to walk a little and without experiencing fatigue, the strength of the grasp was increased, deglutition was more free, and mastication did not, as previously, excite pain. From this time her progress was steady till the 15th December, when the report of the case ends. With the exception of some inclination of the head backwards and approximation of the chin to the breast, together with considerable stiffness of the neck, which allowed only very limited rotation, the child at that period enjoyed perfect health. She was plump and stout, and ran about and played with her companions without experiencing fatigue (xxiv. 520).

2. The second case, which is related by *Dr Martin Solon*, occurred in the wards of M. Récamier at the Hotel-Dieu. The patient, a female, forty-eight years of age, five months before being admitted into that hospital, was seized with headach, particularly in the hindhead. Three months afterwards the pain descended to the nucha; and at the same time the neck began to bend a little to the right side and the head towards the left; while the head could not be rotated on the cervical vertebræ on account of the severe pain excited by the motion. On her admission into the Hotel-Dieu M. Récamier, under the impression that she had a neuralgic affection with muscular spasm, resorted to warm baths, emollient poultices, narcotic fomentations, and finally percussion of the neck with the hand. A month after her admission passed without any change, when she began to complain of pain in the throat, and to speak through her nose. Considerable redness of the mucous membrane of the mouth and some swelling of the tongue were also remarked; but no ulceration or swelling in the throat, no fetor of the breath, no disease of the nostrils; and she insisted she never had any venereal disorder. The same remedies were persevered in for some time longer, but without any advantage. At length, about the middle of September, Dr Solon, on carefully examining the throat, discovered a round tumour as big as a hen's egg proceeding apparently from the middle of the vertebral column, extending anteriorly so as to press forwards the posterior velum of the palate, filling up the whole space between the spine and posterior opening of the nostrils, contracting considerably the isthmus of the fauces, and threatening to press speedily on the base of the tongue and epiglottis. Fluctuation could be perceived over the whole extent of this tumour. The medical attendants of the patient seem now for the first time to have abandoned the notion originally entertained of the neuralgic nature of her complaint, and to suspect the real nature of her case. This alteration of their views, however, made little change in their treatment. The established method of treatment by issues or the cautery, it was thought, would torture the woman without giving her any chance of recovery; and as to the abscess in the pharynx, it was considered that an opening in it would merely occasion a constant troublesome trickling of matter down the pharynx, and perhaps even danger from the matter getting into the glottis, and was therefore admissible only in case it was called for to prevent impending suffocation. Opiates, accordingly, were alone ordered, under the impression that the patient was doomed to certain death. From this time deglutition and articulation became daily more difficult; in the beginning of October a sense of numbness was felt in the right arm; and a small fluctuating tumour was formed over the left parietal region, which was surrounded by an eroded aperture of the bone, and which, on being compressed, disappeared with the production of stupor. The woman, when now urgently pressed, withdrew the denial she had repeatedly given to the assertion of her physician that she had syphilis, and admitted having had chancres and having been cured without mercury. Soon afterwards she was unable to articulate at all, or to swallow either solids or fluids; and the movement of her head occasioned so much pain that she could not even make the ordinary signs of assent and dissent. At length she suddenly fell into a state of asphyxia during the night, and died before the surgeon on service could be summoned to open the abscess.—On dissection, the abscess on the skull was found full of sero-puriform matter, and communicating

through the cranium with the outer surface of the dura mater, which was entire. The tumour in the pharynx extended from the middle of the anterior part of the vertebræ to the left condyloid process of the occipital bone. It was bounded by the pharynx before, the vertebral column behind, and the remains of the muscles laterally, and was lined inside by a thick adventitious membrane. Its contents amounted to three or four ounces, and consisted of a grayish, serous, inodorous fluid, which lay in contact with the lateral parts of the upper cervical vertebræ. The left superior articulating process of the second vertebra was eroded and its cartilage gone; the left lateral portion of the atlas had disappeared, so that there was not a vestige of either the inferior or superior articulating process of that side; and the left condyloid process of the occipital bone was eroded and its cartilage destroyed. On the right side there was less disease, the cartilages of the occipito-atlantal articulation being destroyed, but the bones uninjured; and in the articulation of the atlas with the second vertebra, there was merely redness of the synovial membrane, and slight softening of the articulating cartilages. The lateral parts of the *processus dentatus* of the second vertebra were also somewhat eroded. The transverse ligament of the atlas was entire, so that there was no luxation, and consequently no compression of the spinal cord. The occipital nerves and vertebral arteries were not diseased. The eroded parts of the bones did not present any appearance of softening. The whole cerebro-spinal system was healthy. Caries was found in two other places where its existence was not suspected during life,—in the sixth rib, and in the fifth lumbar vertebra.

The general observations appended by the authors to these interesting cases do not call for particular remark. In both there was remarked, as in other cases mentioned by previous writers, deviation of the cervical spine first from the affected side and then towards that side, which alteration is probably with justice ascribed by M. Ollivier to the affected joint, being, in the first instance, in a state of tumefaction from inflammation, before erosion and absorption of the articulating cartilages take place. The following recent authors on the subject are referred to. *Schupke*, de Luxatione spontaneâ atlantis et epistrophei, Berolini, 1816.—*Rust's* Arthrokakologie, Wien. 1817, of which work the chapter on spontaneous dislocation of the cervical vertebræ has been translated in the nineteenth volume of the *Archives Générales*.—*Ollivier*, Traité de la Moelle Epinière, 2ème édition, 1827. *Bérard, Jeune*, de la Luxation Spontanée de l'occipital sur l'atlas et de l'atlas sur l'axis. Thèse Inaugurale. Paris, 1829.—*Gros*. Thèse inaugurale,—exact title not given. Paris 1830.

OBSTETRICS.

On Difficult Parturition arising from Exostosis of the Bones of the Pelvis. (Dissertatio Inauguralis, Heidelbergæ, 1830; *exhibens casum rarissimum Partus qui propter exostosin in pelvi absolvi non potuit, &c. Auctore Eli de Haber.)*—In this inaugural essay, published by Dr Eli de Haber, under the auspices of Professor Naegele, the author commences with a sketch of Professor Naegele's arrangement of the different kinds of labour, then enumerates the chief cases to be found in medical records of difficult labour arising from exostosis of bones of the pelvis, and terminates with the particulars of a very striking example, where this disease existed to an enormous and unprecedented extent. He observes, that difficult labour from exostosis is very rare, and even much rarer than is commonly imagined. A short notice of an example has been given by *Pinæus*, where the exostosis was attached " to the left *os pubis*, near the lower end of its synchondrosis."—(Opusc. Physiolog. Paris, 1557. Lib. ii. c. 5.) Another is given by *Ruleau*, in his Traité de l'Operation Césarienne, Part i. c. 9, published at Paris in 1704.—In this instance delivery being rendered impossible by an exostosis at the middle and lower part of one of the *ossa pubis*, together with curvature of the *os ilium*, the Cæsarean operation was resorted to with success. *Mierry* relates a third case, where the head of the fœtus was perforated to effect delivery, without saving, however, the life of the mother. Here the exostosis arose from the middle and side of the last lumbar and first sacral vertebræ, was of the form of a chestnut, and encroached nine lines on the shorter diameter of the pelvis, leaving only two inches and a-half of free space.—(De partu difficili a mala conformatione pelvis. Argent. 1764. P. 16.) *Van Döveren* describes a case of exostosis of the upper part of the sacrum, about the size of half a hen's egg, where delivery was accomplished with difficulty, and the woman died.—(Specimen Observ. Academicarum. Gron. 1765, c. 12.) In the Francfort journals for April 1778, an instance is shortly recorded by Nagel, where the exostosis was attached to the inside of the *os sacrum*, and contracted the shorter diameter of the pelvis to half its natural breadth. Lastly, *Madame Boivin* has lately mentioned in brief terms two cases which occurred in her extensive practice at Paris, in one of which there was exostosis on both sides on the anterior surface of the sacro-iliac synchondrosis, and in the other exostosis of the same part on the left side only, so extensive as to fill up a third part nearly of the outlet of the pelvis. —(Memorial de l'Art des Accouchemens, 1824.) Dr de Haber rejects an extraordinary case related by *Plessmann* as either fabulous or jocular. The Berlin accoucheur entitles it, " Exostosis of the vagina, cured by the cautery during labour !"—(La Médécine Puerpérale, ou des Accidens de la Maternité. 1797.)

To these is added the following interesting example, which occurred in the practice of Dr Leydig, a late eminent German accoucheur, and the particulars of which came into the possession of Professor Naegele. The subject was 29 years of age. Fourteen years before, she recollected having received a violent fall on the buttocks upon the ice, and suffering in consequence severe pain in the rectum, which gradually went off altogether, but left ever afterwards some difficulty in passing urine or feces. After being five years married, she became pregnant, and enjoyed very good health till about the twenty-seventh week, from which period she was harassed by frequent vomiting, and very great difficulty in discharging feces and urine. On the 27th of April, being then in her thirty-fourth week, she was also seized with pains in the belly, which, however, did not prevent her going out to the fields to work a little. On the 30th, while raising a weight, she felt a noise as of something giving way, and water rushed from the vagina; after which the movements of the fœtus ceased. On the first of May an accoucheur was summoned to her aid, who, on discovering the state of her case, called in the assistance first of Dr Kraus, and then of Dr Leydig, who drew up the narrative. When the latter first saw the patient on the 4th of May, he found her pale, with sunken cheeks, sharp nose, contorted eyes, accelerated breathing, frequent pulse, alternate heats and chills, and frequent vomiting; yet she was able to walk up and down the room during the intervals of the labour-pains. From the vagina a white fluid issued from time to time, mixed with white fragments, occasionally clysters were given without effect, and she could pass only a few drops of urine by violent straining. The abdomen was not much distended, and free of pain. Two distinct tumours could be felt, one on the left side, soft and tender, and extending as high as the navel, the other in the right side, extending to the *scrobiculus cordis*, hard and tense during every labour-pain. On examination by the vagina, the finger, when introduced to the second joint, encountered a hard firm tumour like a child's head, behind and to the outside of the vagina, uneven on the surface, receiving at one point the impression of the finger, and filling up the outlet of the pelvis so completely, that the finger could with difficulty be passed between it and the *symphysis pubis.* On examination by the rectum, the tumour was found to prevent the entrance of more than the first joint of the finger. The medical attendants were of opinion that this tumour was an exostosis, that the left tumour of the abdomen was the distended bladder, and the right the uterus, and that delivery by the vagina in any manner was impossible. The Cæsarean section was therefore considered the only resource; but as the woman appeared sinking, they contented themselves with palliating her sufferings, by making an opening into the bladder, after an ineffectual attempt to pass the catheter. Next day, as she had unexpectedly revived a little, the Cæsarean operation was performed, without any particular accident; but death ensued in five days.

On dissection the abdomen was found to contain a considerable quantity of pus; the small intestines were healthy; the great intestines united by fibrinous effusion; the neck of the bladder opposite the upper edge of the pubis, its fundus two inches and a-half above it, its whole structure healthy, and free of any trace of the puncture; the uterus 6 inches long, and 4 broad, and covered with lymph, which connected it to the lips of the wound in the abdominal parietes, the wound in the uterus 2½ inches long, the substance of the uterus healthy. In the natural site of the uterus, and to its right side, and projecting above its fundus, there was a roundish tumour 4 inches in diameter, which proved to be the rectum distended by feces. The right kidney and ureter were much distended, the left kidney less so, but the left ureter more so than the right. The exostosis was half of it above, and the other half below the upper outlet of the pelvis, and it filled that aperture so completely, that when the pelvis was looked at downwards, no passage could be seen into the lower pelvis, except a narrow one at each iliac fossa; in fact the fore-part of the tumour was only a line and a-half distant from the pubis. Its length was 7 inches, its breadth 6; its upper end was on a level with the intervertebral space below the third lumbar vertebra, its lower end nearly reached the point of the sacrum; and in its internal structure it was cancellated, some of the cells being large, and all lined with a white membrane, and filled with a yellowish fluid.

A case of similar nature requiring the Cæsarean section is given in the present number.

LEGAL MEDICINE AND MEDICAL STATISTICS.

Case of Poisoning with Nux Vomica. (*Glasgow Medical Journal, August* 1830.)—Mr Watt, Member of the Faculty of Physicians and Surgeons, Glasgow, has described an instructive case of Poisoning with nux vomica, of which the following is a full abstract:—An unmarried female, forty years of age, swallowed threepence worth of powder of nux vomica. In the course of half-an-hour she made many fruitless efforts to vomit. Soon afterwards slight convulsive tremors appeared, with intervals of almost complete remission of the spasms. The intervals, however, became rapidly shorter and shorter, and the spasms more and more severe, so that the people about her had difficulty in preventing her from falling out of bed. Medical advice was then thought of, and Dr Watt was called,—about an hour after the poison was taken. He found her on the floor, stretched on her back, with the arms and legs extended and apart from one another, her countenance anxious, but her mind entire. He immediately tried to give her by teaspoonfuls at a time a solution of two scruples of sulphate of zinc; but finding she could not swallow, and thinking this might arise from her peculiar position, he proceeded to raise her. While in the act of doing so, severe spasms were brought on. "The neck and back were thrown into a state of tetanic stiffness and turned backwards, the arms thrown out from the body and forcibly extended, the hip and knee-joints extended and so stiff that flexion was utterly impossible. The countenance became at first exceedingly anxious, expressing a state of suffering; then the eye-balls became fixed, and seemed as if about to start from their sockets, with the jaws locked, and the lips violet-coloured, from respiration being obstructed by spasm of the muscles of the chest, which, with those of the abdomen, were hard and tense, whilst the whole body was agitated by a tremulous movement. Being laid down, the spasmodic action diminished in two or three minutes. She was still unable to swallow, but, anxious for relief, requested to be turned slowly round upon her side, the attempt at which brought on a return of the tetanic spasms. On laying her again on her back, the respiration could not be perceived, and the pulsation of the heart was gone, although from slight tremulous movements life did not seem to have left her entirely for nearly a minute."

The body was examined sixty hours after death. The vessels of the pia mater were not distended. There was a good deal of serosity on the surface, and upwards of a tea-spoonful in each lateral ventricle, of the brain. The substance of the brain was soft; the lumbar part of the spinal chord was much softer than the cervical, and the veins on the posterior surface of the cord were gorged for an extent of two inches in the loins. The lungs were less infiltered than usual, the heart flaccid, the ventricles and auricles completely empty of blood. The inner membrane of the stomach was easily torn, and marbled; and it contained a pint of thick, dark fetid fluid. The duodenum was distended and its muciparous glands enlarged. The matter found in the stomach was examined by Mr Graham, lecturer on chemistry. "The extract was intensely bitter, and had the smell of liquorice, a solution of it was precipitated by ammonia, but not reddened by nitric acid." Mr Graham says he is more inclined to rely on the taste and smell, than on the chemical properties of the drug, as evidence of its adminstration. Reddening, however, is not the effect produced on its infusion by nitric acid. The colour produced is a lively yellow, or intermediate between this and orange. No satisfactory experiments have yet been made on the best mode of detecting this substance in cases of suspected poisoning. But indeed the peculiar symptoms and their usually short course, will in general furnish sufficient evidence of poisoning with nux vomica, even without a chemical analysis.

Case of Poisoning with Vapour of Nitrous Ether.—(Midland Medical and Surgical Reporter, No. ii.).—A Druggist's servant went to bed in good health, but not rising at her usual hour next morning, nor when the family went to call her, the door of the apartment was broken open, and she was found dead. She lay on her side, with her arms folded, the countenance and posture composed, and the whole appearance that of one in a deep sleep. The body was examined by three surgeons, who found she was three months gone with child. On further inspection of the alimentary canal an inflamed appearance was perceived in the stomach, and in consequence, suspicion seems to have been at one time entertained that poison had been swallowed. The nature of the case, however, was fully explained by the discovery in the apartment of a broken jar which had contained above three gallons of the spirit of nitric ether. The medical men very properly gave their opinion that death had been occasioned by the woman inhaling the air of a confined room strongly impregnated with the ether.

The Editor of the Journal in which this notice is contained, observes that he is acquainted with an instance where effects of the same discription, though not absolutely fatal, were produced by the inhalation of an atmosphere loaded with the vapour of sulphuric ether. The individual, a young man, fell into an insensible state, remained apoplectic for some hours, and would undoubtedly have perished had he not been removed in time from the noxious atmosphere. Dr Christison quotes a somewhat similar instance, that of a gentleman who, in consequence of inhaling sulphuric ether too long, was attacked with intermitting lethargy for thirty-six hours, depression of spirits, and lowness of pulse. There can be no question, then, that the inhaling of air much loaded with these vapours will prove highly dangerous.—We regret that the account of the case in the Midland Reporter is so meagre. It is taken by the Editor from a coroner's inquest, where the investigation appears to have been conducted, as often happens, in a slovenly manner. The only organs which were examined, so far as the account of the inquest goes, were the stomach, uterus, and lungs. We have already mentioned the state of the two former organs. The lungs were much gorged.—One of the medical witnesses says he was unable to account for the appearance of inflammation of the stomach. We have not the slightest doubt that the supposed inflammation was nothing more than vascular turgescence originating in the manner of death. The woman seems to have died, as in cases of poisoning with carbonic acid, from slow obstruction of the breathing,—from gradual asphyxia; and in no other circumstances is it usual to find such extensive and intense congestion of the mucous membranes. Sometimes in fact the congestion amounts in the intestines to the formation of large extravasated patches, several square inches in size.

1. *Compound Poisoning with Arsenic and Laudanum.* (*London Medical and Physical Journal, October* 1830. *lxiv.* 295.)—Mr Jennings, Surgeon at Leamington Spa, has related in the Journal now quoted an interesting case of compound poisoning, where the individual swallowed at the same time two drachms of arsenic and three ounces of laudanum. Each poison seems to have in some measure modified the operation of the other; but the symptoms of poisoning with laudanum were at length developed and proved fatal, while the usual effects of arsenic ceased altogether. When Mr Jennings first saw the patient, four hours after the poisons were taken, there was no pain or burning in the throat, stomach, or bowels, no tenderness of the abdomen, and no particular stupor. She complained merely of being tired and sleepy, from the violent action of an emetic about two hours previously. The eyes were blood-shot and heavy, the pupils contracted, the pulse 100. But the usual symptoms of arsenic were so completely absent, that two medical men who saw her an hour after the poisons were taken, could not be persuaded that she had really swallowed arsenic. Mr Jennings immediately ordered a repetition of the emetic, and then venesection, leeches, blistering, and the cold affusion. The patient was at the same time kept walking about. In three hours and a-half after he first saw her, the drowsiness increased, so that she was with great difficulty kept awake. About this time, too, she once or twice complained of some pain in the bowels; but she had no tenderness, or pain in the stomach; and the bowels were only once moved. In half an hour more she became comatose, and in an hour afterwards she expired in a state of profound coma, with dilated pupils and laborious breathing.

The body was examined sixteen hours after death. The dura mater was vascular, the sinuses gorged, the pia mater vascular, the large veins filled with a treacly-like blood, the ventricles free of fluid, the brain firm, and its bloody points unusually numerous. The stomach was externally healthy, the ileum interspersed with a few vascular patches. The stomach contained half a pint of fluid. Its villous coat was generally pale; but at the great arch there were two small red patches. The mucous coat of the duodenum presented a diffuse pink blush, that of the jejunum many bright red patches, that of the ileum several similar patches, that of the great intestines nothing worthy of note. In the chest, the heart was found rather flaccid, and the great vessels not particularly gorged with blood, which was very dark.

On analysis, it was found that the fluid in the stomach was turned yellow with sulphuretted hydrogen, and yielded a scanty yellow precipitate with the ammoniacal nitrate of silver. From the contents of the small intestines the precipitates procured were large enough to yield visible metallic arsenic by the process of reduction. The matter vomited after Mr Jennings saw her, and fully four hours and a-half after the poison was swallowed, likewise yielded a quantity of precipitate sufficient for the test of reduction. There could be no doubt, therefore, that the arsenic had been swallowed.

Poisoning with Nitrate of Mercury. (*Medical Gazette,* 11*th December* 1830.)—The following case, by Dr Bigsley of Newark-upon-Trent, is the first instance published, so far as we are aware, of poisoning with the nitrate of mercury. It was taken in the form of a strong solution, with large excess of acid, prepared by "dissolving seven parts of mercury in eight parts of the acid," and adding a little verdigris to communicate a green colour. Of this solution about a tea-spoonful was swallowed at a quarter past nine in the evening, by a butcher's apprentice, for the purpose of committing suicide. He had taken a quart of ale not long before. Soon afterwards he complained of being very ill, was seized with vomiting, and rolled about on the floor calling out for a knife to cut his throat, and put him out of pain. A medical man was soon procured, who found him complaining of pain in the mouth and throat, and retching violently. The countenance was pale and anxious, the extremities cold, the pulse labouring and sometimes indistinct, the bowels rather loose. The stomach was immediately cleared out with the stomach-pump, and chalk administered. When Dr Bigsley first saw him, about an hour after the poison was taken, he was more composed, the face pale and bloated, the eyes wild and heavy, the lips livid, the pulse 120, small, but regular. He complained of a sense of burning from the mouth downwards along the gullet, to the stomach and abdomen, the whole of which was tender, and in the epigastric region tense. The vomiting and purging continued to recur, and did so till his death, which took place near midnight, without any new symptom, and in particular without his mind having been at all impaired.

The body was examined in twelve hours. The face was bloated and bluish, the lips livid, and covered with froth, the bodily warmth still not exhausted. The whole alimentary canal contained chalk in powder. The back of the tongue was hard and rough, and presented one small vesication; there was a similar blister on the epiglottis; and the larynx and trachea were red and injected. The pharynx was of a deep rose-red tint, with small purple blotches, and some hard, rough, brown, irregular crusts of the size of a horse bean, interspersed. The brown marks were obviously imperfect eschars. These signs of irritation became less distinct downwards; about three inches of the middle of the gullet were healthy; but under this part the same appearances recommenced. The stomach outwardly presented no appearance of disease. Its coats were thickened, especially near the pylorus. It was almost empty,—containing only a few ounces of water tinged with bile, and some brown grumous matter. The whole villous coat was of a deep rose-red colour, with some patches several inches square, near the cardiac end, of a dark livid or brown colour, and of the appearance of eschars. Some of them were in the same state of progress with the brown marks on the pharynx; others were softened into a brown pulp; and when these were rubbed off, the surface beneath was left smooth and bright-red. The eschars were chiefly situated on the summits of the rugæ. There was no abrasion of the membrane, except where the eschars were rubbed off. The duodenum was similarly affected with the stomach, but less violently. The other intestines had a dull red blush externally, arising from a general red suffusion of their internal coat. The deep redness and lividity re-appeared in the *caput cœcum*, and from this quarter diminished in intensity towards the rectum, which was sound.

The other abdominal organs, as well as the viscera of the chest, were in the healthy state. The head was not opened.

The nitrate of mercury is known to be, like most of the soluble salts of that metal, a powerful corrosive; and the particulars of the present case constitute an excellent illustration of the fact. The violent effects of so small a quantity of the poison are remarkable, and seem to have surprised the author of the case. But ravages of equal extent have been witnessed from an equally small dose of corrosive sublimate.

On the effects of Oil of Bitter Almonds and other Oils containing Hydrocyanic Acid, when that acid is removed from them. (*Rust's Magazin für die ges. Heilkunde*, 1830, xxxii. 494.)—When the essential oil of the Cherry-laurel, Bitter almond, or Bird-cherry is digested with solution of caustic potass, the solution when properly tested is found to contain hydrocyanic acid. The oil which remains preserves when distilled its peculiar odour, and commonly retains a certain proportion of hydrocyanic acid; which, however, may be entirely removed by repeating the process of digestion with potass once or twice. The oil eventually procured retains its peculiar odour. According to some recent experiments mentioned by *Orfila* in a new edition of his Lectures on Medical Jurisprudence, this purified essential oil continues as poisonous as ever; but he admits, that the experiments which led him to this conclusion were not sufficiently extensive to allow him to make his statement with confidence. *Dr Göppert* of Breslau has taken up the subject, and arrived at results opposite to those of the Parisian Professor; for he finds, that when any of the essential oils mentioned above is treated with caustic potass till the alkaline solution ceases to yield a blue precipitate with the ferrocyanate of potass, it ceases to possess any poisonous property except what is equally possessed by all essential oils, such as oil of turpentine. He performed various experiments with the crude oils of the vegetables mentioned above, and found that sixteen drops of Cherry-laurel oil, containing 2¾ per cent. of hydrocyanic acid, killed rabbits in thirty-five minutes; that twelve drops of the Bird-cherry oil, containing 5¼ per cent. of acid, killed them in twenty-five minutes; and that five drops of Bitter-almond oil, which contained 14¼ per cent of acid, occasioned death in six minutes. But when the same oils were previously deprived of their hydrocyanic acid in the manner already alluded to, twenty-four drops of each, even of the Bitter-almond oil, were given to rabbits in a single dose without the least danger to life: No effects were produced except at times cries, trembling, running about, and hurried respiration, which are equally occasioned by the common essential oils, such as oil of turpentine and oil of lavender.

The author particularly observes, that although the hydrocyanic acid be removed, and along with it the peculiar poisonous property of these oils, they retain altogether their peculiar odour. This fact illustrates and confirms an observation made by Dr Christison in regard to an error, which is extremely common, and which it is of some moment to rectify—the comparing the smell of pure hydrocyanic acid to that of the essential oil of almonds or cherry-laurel water. There is in reality scarcely any resemblance between them, and the experiments of *Göppert* point out the reason. It is essential to bear this in mind in the medico-legal examination of cases of alleged poisoning with the compounds of hydrocyanic acid. In the accounts which have been published of death caused by its various forms, mention is frequently made of a strong bitter almond odour having been perceptible around the body; and this observation is applied to cases of poisoning with pure or simply diluted hydrocyanic acid, as well as to cases of poisoning with the essential oils. Now, in cases of the former description, it is impossible that a bitter-almond odour can be perceived, although some may consider the odour which occasionally occurs to be somewhat similar. But in point of fact it frequently happens that in such cases no smell even of hydrocyanic acid is perceptible near the body, although death has ensued within a minute after the taking of the poison, and when the examination is made in an hour or two afterwards. Of this fact we have satisfied ourselves by many experiments on the lower animals.

Singular Trial for Infanticide. (The Star; Guernsey Commercial and General Advertiser.)—The particulars of the following extraordinary case are contained in the newspaper mentioned above for October 25th, 1830. Their authenticity may be relied on, notwithstanding the well-known inaccuracy of newspaper reports, because in Guernsey, where the old French law is still in a great measure followed, the evidence of the witnesses is fully taken in writing, and the newspaper report appears a *verbatim* copy.

Mr Beasse, a person of tolerable station on the island, carried on an illicit intercouse with his servant, Sarah Elliot, the consequence of which was, that she became pregnant. Early in the month of May, he mentioned the latter circumstance to a friend, and confided to him his intention of having the girl sent to the adjoining coast of France, or to England, when the period of her delivery approached, in order to escape the scandal. On the night of the 10th of June following, however, her delivery took place in his house,—prematurely, according to the statements of the girl herself, and of Mr Beasse in his declaration,—and in all probability at least unexpectedly, for he had a party of friends with him that evening till midnight, and when they left his house he accompanied them, and did not return home till half past one, although he knew she was unwell. Medical advice was not procured. The girl was delivered about four or five in the morning of the 11th of a still-born child, as she represented; and in order to keep the affair quiet, and allow her to recruit her strength, she was locked up in her room, and a false story told of her having gone to visit a brother in the neighbourhood. It is particularly worthy of remark, that among the steps taken to spread this story, the girl herself, within two hours after her delivery, went to the window of her room, and told her pretended intention to a boy in the garden, who stood about forty paces off. About the same time Mr Beasse went to a friend, the same he had consulted formerly, to ask his advice what he should do; and at this person's recommendation he allowed a surgeon to visit the girl at mid-day, who found her in bed with the after-birth not expelled, and who examined the body of the infant, without being able to remark any external appearance of violence. Mr Beasse likewise made inquiries as to the best mode of secretly burying the child, but no means could be devised that satisfied him, by which it might be buried in the church-yard. Various circumstances having excited a suspicion of foul play, the house was visited on the morning of the 14th by a constable with two medical men. Mr Beasse admitted that a child had been born; stated that it had either been still-born, or at least, when he was summoned to the room by his servant, that he found it lying on the floor amidst a quantity of blood, and respiring feebly; and after several attempts at evasion, he conducted the constable to a spot in his garden where he had buried it. The mother, when questioned on the subject, said she was only in her fifth month, that after she was taken in labour, she went to bed and there fainted; and that she was ignorant of the sequel, till she came to herself and found the child on the floor dead. She also stated that baby-linen had been provided for the child; but neither she nor Mr Beasse were able to prove this.

Both Mr Beasse and the servant were indicted for child-murder on the 23d of October last. The general evidence brought forward on the trial was nearly the same with what has just been detailed, with the addition of some of the usual contradictions in the declarations of the prisoners, which it appears unnecessary to enumerate. The medical evidence is very minutely reported as delivered by Messrs O'Brien and Hoskins, who examined the body, and Mr Mauger, who visited the girl on the morning of her delivery. All of them agree that the child was not immature, but apparently born at the full time; and Mr O'Brien adds, in confirmation of this opinion, that the state of the female prisoner on the 14th June, four days after her delivery, corresponded with that of one who had brought forth a nine-month's child. The data in support of these statements are not given, so that we cannot judge of their accuracy. But the opinion founded on the state of the woman is obviously much overstretched; at least we question whether any experienced accoucheur would venture to say, from the state of the mother four days after delivery, whether she had given birth to a seven, eight, or nine month's child.

The body of the child was buried in a napkin, which was much besmeared with blood and meconium; and some florid blood was oozing from the mouth; but no other sign of violence was visible outwardly. On clearing away the blood from the inside of the mouth the surgeons discovered a lacerated wound under the tongue, which separated the frœnum linguæ a considerable distance backward. The lower jaw being then detached from the face, the posterior fauces were seen covered with blood; and when this was removed, two other lacerations were found, one on each side of the palate, extending backwards into the throat; the throat beyond the lacerations was filled with blood, on removing which a fourth laceration was discovered, wide enough to admit the fore-finger, penetrating the upper and back part of the gullet, and laying bare the bodies of some of the upper vertebræ. The cellular tissue between the gullet and windpipe was ecchymosed or infiltered with blood along the whole course of the windpipe down to the root of the lungs. The lungs were high, expanded, and free from putrescence; their colour pale pink and mottled; they crepitated when handled, and when cut gave out air and a little blood; they were very buoyant in water, and when a portion was squeezed firmly it expanded again and still floated. The heart was healthy. The umbilical chord was lacerated about three inches from the navel, and had not been tied. In the abdomen blood was found besmearing the whole viscera. This proceeded from a wound in the rectum, entering from within about two inches above the anus, passing about two inches more between the coats, and then piercing the peritonæal covering; and the course of the wound between the coats was marked by a line of ecchymosis. This wound appeared to have been made with a sharp instrument. The rectum contained scarcely any meconium; but the rest of the great intestines contained the usual quantity.

From these facts, both of the medical gentlemen agree that the child was born alive and breathed, and that it had been put to death by means of some instrument introduced into the throat, and also into the anus. They deny that it is possible to produce such injuries after death, even immediately after it; they maintain that, in the circumstances of the case, the hydrostatic test, together with the ecchymosis in all the wounds, is certain proof of the child having been born alive; they are of opinion that the wounds might occasion speedy death, but that on the whole, the wound in the anus was not calculated to produce death with great rapidity, and that those in the throat, though attended with ultimate danger, could not prove immediately fatal, except by the concurrence of suffocation, probably produced at the same time by the rude means employed. In particular, however, they express great doubts whether the woman could have inflicted these wounds herself immediately after delivery. In answer to a query as to this point, Dr O'Brien says, "the word *possible* has so wide a range, that I would beg leave not to answer it; but a person in the situation of Sarah Elliot, suffering mentally and bodily, as she then must necessarily have done, weltering in her blood, and only in part delivered, I would almost say must be more than human to possess the strength of purpose and steadiness of hand necessary to perpetrate at that time the excesses committed on that child." Mr Mauger also thinks it almost impossible. Mr Hoskins waived replying to the query, on the ground that he had not seen the woman before or after her delivery.

In opposition to this evidence against the prisoner, Mr Curtis, a surgeon, was examined on the opinions given by Messrs O'Brien and Hoskins, and states,—that the child might have died of hemorrhage from the untied umbilical cord,—that it might breathe in the passages before delivery so as to expand the lungs as remarked by the inspectors,—that he has known women resume their daily occupations on the very day of their delivery; and has no doubt some are in such a state as to be able to kill their child;—that if a woman, as in the prisoner Elliot's instance, is brought to bed at four or five in the morning, in two hours is able to go to her window and talk to a person forty yards off so as to be heard, and on the same day resumes her daily occupations and washes her room, she would be able to destroy her child immediately after delivery;—that he has actually met with instances of women who immediately after delivery were so strong that they could have destroyed their children;—that the wounds inflicted in the prisoner's child required little force to inflict them;—that ecchymosis may be produced on the dead body.

Such was the evidence. Both prisoners were found guilty. Mr Beasse was found guilty as principal, and sentenced to death, which sentence was subsequently put in execution; and Sarah Elliot being found guilty as accessory, was sentenced to make the *amende honorable* before the court, and then to six years' banishment from Guernsey.

We have but a few remarks to add to this interesting narrative. It is clear the child was born alive. This is proved by the state of the lungs, with the ecchymosis and internal hemorrhage, which, notwithstanding Mr Curtis's opinion, could not have occurred after death. It is also clear that the child was put to a violent death, the wounds and hemorrhage being sufficient to account for it, and such as put accidental death quite out of the question. The only difficulty in the case was to fix on the actual murderer. One or other of the prisoners certainly was the murderer; for no one else was in the house at the time. But it is by no means so clear to our minds, as it seems to have been to the court, which of the two was the guilty person. Notwithstanding the opinion of Mr O'Brien and Mr Mauger, there cannot be a question among well-informed medical jurists, that the female prisoner might have killed the child herself immediately after delivery, as Mr Curtis very correctly maintained. Such being the case, we do not see any evidence to prove who was principal and who accessory; or rather,—for the difficulty now stated would in *British* law be no impediment to inflicting the full pains of law on both as murderers,—we see no satisfactory proof in the evidence, that there was an accessory at all, and no proof which of the two did the deed. If we suppose that either one or the other murdered the child, there is nothing in the conduct of the remaining party, or in the evidence generally, which might not be perfectly well explained on the principle that he or she acted with the view of keeping quiet what the murderer was endeavouring to pass off as the delivery of a still-born child. We are of opinion, then, that the court acted hastily in executing the prisoner Beasse, and feel convinced that he would have escaped either in an English or Scotch court of justice.

Bills of Mortality of the City of Berlin for the year 1829. (*Journal der Praktischen Heilkunde, Mai* 1830.)—This is the most complete statistical account we have seen of the causes of mortality in a great city since the excellent table by Dr Roberton of the diseases of Manchester, published in a late volume of this Journal.

The *births* were 8450, of which 4287 were boys, 4163 girls,—649 boys and 666 girls born out of wedlock, or a proportion of 1 to $6\frac{2}{5}$,—228 boys and 179 girls born prematurely, or still-born, or a proportion of 1 to 20 of the total births, (and 1 to 12 out of wedlock,)—94 cases of twins, and 1 case of three at a birth. The *marriages* were 1988. The *deaths* were 6884,—of which 3721 were males, 3163 females,—and 3213 under, 3671 above ten years of age. The following table presents a view of the number of deaths at different ages:

	Male	Female	Tot.		Male	Female	Tot.
1st year,	1064	926	1990	From 20 to 30,	333	195	528
2d do.	292	246	538	— 30 — 40,	311	197	508
3d do.	138	130	268	— 40 — 50,	353	211	564
4th do.	66	86	152	— 50 — 60,	327	245	572
5th do.	43	40	83	— 60 — 70,	321	282	603
From 5 to 10,	99	83	182	— 70 — 80,	208	294	502
— 10 — 15,	30	30	60	— 80 — 90,	74	125	199
— 15 — 20,	53	53	106	— 90 —100,	9	18	27

Above 100, 2 women.

The next table is a very important one, containing the names of the diseases in every case of death, the males and females being distinguished, and also the cases below and above the tenth year of life.

	Adults.		Children.		Total.
	Males.	Females	Males.	Females.	
Weakness of old age,	218	388	—	—	606
Weakness at birth,	—	—	120	.12	232
Still & premat. births,	—	—	228	179	407
Teething,	—	—	89	70	159
Tetanus,	9	—	9	8	26
Spasm of stomach,	1	4	—	—	5
Asthma, *	7	12	4	10	33
Convulsions,	7	9	412	381	809
Scrofula,	3	2	13	14	32
Fungus, †	—	—	1	6	7
	1	—	9	2	12
Hydrocephalus,	1	—	80	53	134
Catarrh and chincough,	—	—	15	18	33
Small-pox,	18	7	2	4	31
Measles,	—	—	5	3	8
Red measles, ‡	—	—	4	2	6
Scarlatina,	7	4	33	30	74
Croup,	1	—	17	21	39
Erysipelas,	1	2	1	3	7
Inflam. brain,	21	10	64	50	145
——— lungs,	35	44	64	50	193
——— belly,	15	29	10	4	58
——— liver,	5	9	—	1	15
——— intestines,	3	3	—	—	6
——— larynx,	1	4	12	1	21
——— stomach,	2	2	1	—	5
——— heart,	2	1	1	—	4
——— bladder,	1	—	—	—	1
——— womb,	—	2	—	—	2
——— chest,	4	4	6	4	18
Inflamm. fever,	6	2	5	4	17
Nervous ——	108	61	12	10	191

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* Brustkrampf. † Schwämmen. ‡ Rötheln.

	Adults.		Children.		Total.
	Males.	Females.	Males.	Females.	
Bilious	5	2	—	1	8
Mucous	4	3	2	1	10
Putrid & petech. fever,	1	1	—	—	2
Intermit.	3	3	—	2	8
Puerperal	—	7	—	—	7
Slow	313	282	288	290	1173
Consumption,	407	175	2	1	585
Phthys. laryng.	19	22	—	—	41
Tabes mesent. [*]	9	8	—	2	19
Chronic hepatitis, [†]	9	8	1	—	18
Ulceration intest. [‡]	5	2	—	—	7
Catarrh of bladder,	1	—	—	—	1
Dropsy,	134	107	23	19	283
Hydrothorax,	79	41	6	9	135
Jaundice,	2	1	4	2	9
Diarrhœa, [§]	8	1	12	17	38
Cholera,	2	2	11	4	19
Hemorrhage, [‖]	14	11	—	2	27
Hæmatemesis,	13	3	1	—	17
Apoplexy,	308	205	101	85	699
Drunkenness,	2	—	—	—	2
Morbus cœrul.	—	1	—	2	3
Diseased urethra,	1	—	—	—	1
Childbed,	—	6	—	—	6
Organic dis. of abdomen,	4	3	1	—	8
Do. of heart,	12	17	1	1	31
Insanity,	2	3	—	—	5
Hernia,	3	10	—	—	13
Caries,	3	2	4	—	9
Cancer,	2	1	1	—	4
Cancer of womb,	—	38	—	—	38
——— breast,	—	4	—	—	4
——— stomach,	1	—	—	—	1
Gangrene,	6	7	—	1	14
Asthma,	5	3	—	1	9
Emphysema,	1	—	—	—	1
Gout,	3	3	—	—	6
Stone,	1	1	—	—	2
Obstructed intest.	1	2	—	—	3
Indurated cell. tiss.	—	—	3	3	6
Tabes dorsalis,	2	1	2	1	6
Surgical operations,	—	1	—	—	1
Softening of stomach,	—	—	8	6	14
Scirrhus of do.	7	4	1	—	12
Metrorrhagia,	—	2	—	—	2
Suicide,	39	13	—	—	52
Accidents,	68	21	9	6	104
Unascertained diseases,	44	27	4	10	85
Total,	2020	1653	1702	1509	6884

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[*] Unterleibschwindsucht.

[†] Leberkrankheit and Leberverhärtung.

[‡] Darmschwindsucht.

[§] *Comprehending two articles of the German table, namely,* Durchfall—Ruhr.

[‖] Blutsturz.

Edinburgh, 15*th November* 1830.

This day the Royal College of Surgeons of Edinburgh resolved, That, in addition to the course of study prescribed by their regulations enacted in 1829, it shall be required of all persons who commence the study of medicine after the 1st August 1831, that they produce certificates of having attended a three months' course of medical jurisprudence, before being admitted to examination for a surgeon's diploma.

The course of study, with this addition, will comprise the following separate and distinct courses of lectures:—

		Duration at least
Anatomy,	2 Courses,	Six months each.
Practical Anatomy,	1 Course, or 2 Courses,	Six months; Three months each.
Chemistry,	1 Course,	Six months.
Practical Chemistry, The number of pupils in each class being limited to 25.	1 Do.	Three months.
Materia Medica and Pharmacy,	1 Do.	Six months.
Institutions of Medicine, or Physiology,	1 Do.	Six months.
Practice of Medicine,	1 Do.	Six months.
Clinical Medicine, During the period of attendance at the hospital where they are delivered.	1 Do. or 2 Courses.	Six months; Three months each.
Principles and Practice of Surgery,	2 Courses,	Six months each.
Or Do. and Military Surgery,*	1 Course, 1 Do.	Six months. Six months.
Clinical Surgery, During the period of attendance at the hospital where they are delivered.	1 Do. or 2 Courses,	Six months; Three months each.
Midwifery, and Diseases of Women and Children,	1 Course,	Three months.
Medical Jurisprudence,	1 Course,	Three months.

The candidate must also have attended for *eighteen* months a public general hospital, containing at least eighty beds; or for *twelve* months such a *public general hospital*, and *six* months a *medical* or *surgical hospital*, or *dispensary*, recognized by the College on special application.

The period of attendance on these classes, &c. required of each candidate, is *four* winter sessions; or *three* winter and *three* summer sessions, provided that in each summer session he shall have attended one or more of the courses prescribed or recommended by the College, exclusive of hospital attendance, and also provided that the summer courses of lectures shall not commence till after the conclusion of the winter courses.

Every candidate is also required to produce evidence of having received regular instructions in the elements of mathematics; and of having attended a course of mechanical philosophy, of at least three months' duration, delivered by a professor of that branch in a university, a lecturer in a public institution, or a teacher specially recognized by the College.

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* The course of military surgery must be delivered by a professor of that branch in a university; or by a lecturer, who, in addition to the other required qualifications, has served in the medical department of the army or navy; and the course of lectures must be of at least six months' duration, lectures being delivered at least thrice per week.

The mathematics and the mechanical philosophy may have been attended by the candidate either previously to or during his *professional* education.

For more particular information regarding the course of study, qualifications of teachers, privileges of the schools of medicine, registration, &c. reference is made to the regulations of the College enacted in July 1829, of which copies may be had on application to the Secretary of the College, or to the officer.

By authority of the Royal College of Surgeons of Edinburgh,
John Gairdner, M. D. *President.*

Plan of the Edinburgh Medico-Chirurgical Society for collecting and reporting Cases in Medical and Surgical Practice.—We beg earnestly to call the attention of our readers and of practioners at large to a plan which has been lately proposed to the Medico-Chirurgical Society by its president Dr Abercrombie, and unanimously adopted by its members, for promoting and improving the publication of detached cases in the practice of medicine and surgery. It has always been a matter of deep regret that the professional experience of a vast proportion of practitioners is lost to medical science,—partly because many cannot spare the time for digesting the results of their practice in a form worthy of the station and celebrity they have attained,—partly because they feel diffident as to their power of producing regular dissertations fit for the public eye,—but chiefly because, in private practice especially, new facts and important observations occur at such a distance of time from one another, and in so disjointed an order, that the particulars of them are apt to escape the memory long before they have accumulated sufficiently to form a collection capable of being collated and arranged. To remedy this evil, Dr Abercrombie has proposed that the members and friends of the Medico-Chirurgical Society shall be invited to contribute individual cases or sets of cases at the time they occur, drawn up succinctly, and without the expenditure of labour usually required for regular and studied essays. When the contributions have accumulated sufficiently, it is proposed that they shall be reviewed from time to time by select committees; the cases themselves or the facts arising out of them classified and generalized; and a report published occasionally, consisting of the particular facts with their observers' names, and the general conclusions to which, in the opinion of the redactors, they may appear to lead. The plan has been already commenced and acted on by many of the Edinburgh members with great spirit; and from what has been already done, we are convinced that the practical results will surpass the most sanguine expectations of the originator of the measure, provided his proposal meets with the support, among practitioners both in town and in the country, which, in common with him, we confidently anticipate. We believe that many will be glad to contribute their share to this undertaking, whose thoughts would otherwise never turn towards the troubles of authorship; and we are sure there is not a man of the least respectability in the profession who will not find annually many opportunities of adding valuable facts to the general store.

When the contributions have accumulated sufficiently to admit of classification, we believe it is the intention of the Society to publish their Reports of the Cases in a cheap form, so as to allow of their being extensively circulated. By these means, it will be in the power of the Society to present to their brethren in country situations easy access to a treasure of pathological and practical truths, which can only be furnished by a numerous association of individuals, and which at the present moment exists in no publication with which we are acquainted. Communications may be addressed to Drs Alison and Gregory, the Secretaries, or to any of the members of the Society resident in Edinburgh.

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